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FUNDAMENTALS OF PSYCHOLOGY: THE PSYCHOLOGY OF THINKING

Conference Editor: ERNEST HARMS

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* This series of papers is the result of a conference entitled *Fundamentals of Psychology: The Psychology of Thinking* held by The New York Academy of Sciences on April 28 and 29, 1960.

Part I. Theoretical Aspects of the Psychology of Thinking

INTRODUCTION: UNITED STATES PSYCHOLOGY OF TOMORROW

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During those weeks last year when the ideas for the meeting on which this monograph is based first crystallized, my thought was again and again drawn back to an address made by one of the inaugural speakers during the International Congress of Psychology at Oxford, England in 1923. At that symposium international relationships and personal contacts among psychologists interrupted by the First World War were resumed, and the political turmoil that followed it had diminished. The speaker, with great sincerity and emotion, emphasized the role that psychology, as a science of man's inner and personal life, ought to assume in the prevention of a recurrence of a world war. "Psychology"—the words echo in my ears—"is about to become the basic one of all the sciences; firmly established knowledge about ourselves will give us controls over ourselves and our human relations. Wars will no longer be possible if the sciences dealing with human thoughts, emotions, and will are properly developed and applied."

We all know how this hope was disappointed. Psychology did not prevent either the political turmoil in the Europe of the 1920s with its leftists, rightists, mass movements, and dictatorships, or the Second World War. It did not at any time achieve the proudly asserted over-all scientific influence or assume the role of interscientific balancer that was being predicted by many in those years. In sobering contrast to this widespread hope there appeared, in 1927, a book by Karl Buehler entitled *Die Krisis der Psychologie* (*The Crisis of Psychology*). The radical honesty of this book struck like a bombshell all those who were seriously concerned with any phase of the science of psychology in Europe. Karl Buehler refrained from calling the situation a Tower of Babel; instead, he spoke of an *Aufbau Krisis* (developmental crisis) and of an *embarras de richesse*. There was an outpouring of mass unrelated individual studies; there were battles between individuals, groups, and schools who had not arrived at an understanding of each other's terminology; and there was a complete lack of a view of the field of psychology as a whole—of its autonomy, fundamentals, or teleological aim. For most of those who adhered to a specific theoretical point of view, one of the major conclusions of Buehler's analysis was astonishing and even embarrassing. Buehler maintained that the only point of view that might make psychology a really unified science was that held by the United States "behaviorists," a view that he hoped would soon be brought to Europe to conquer the fortress of psychology there.

In 1928, the year after the publication of Buehler's book, I came to the United States to find out for myself what I could learn about behaviorism to supplement what I had learned from the few books on it that had been available in Europe. The hope Buehler had put into my traveling bag, that I would find this a basically sound and united psychology, flew out of the window as

soon as I entered the first department of psychology and received my first orientation. The old ambassadors to Leipzig and Würzburg, who had supposedly inaugurated psychology as an experimental science in the United States, were in mothballs in the "emeritus" attics. In almost every psychological institution there was indeed much talk about behaviorism, but there was no uniformity of theory or terminology. There were different groups or schools—at Johns Hopkins, Yale, Columbia, Chicago, Berkeley, and Stanford—that were happily engaged among themselves in controversies that to a birds-eye viewer like myself made the whole thing appear not much different from that non-co-ordination amounting almost to disintegration that described the status of psychology in Europe. There was hardly anywhere apparent an impulse to co-ordinate, and to find minds like that of Edna Heidbreder, who a short time later wrote her *Seven Psychologies*, required a Diogenes' lamp. In some quarters one even received the impression of an attempt to reverse Buehler's contention—to look outside for aid. However the psychological argonauts like William MacDougall and Carl Spearman who were called to these shores made hardly a dent. Although there was an amazing growth within the field, with new applications appearing almost every day, there was no impulse to achieve real co-ordination. The participant in a psychological conference in the early 1930s could not but confess some confusion. This confusion became an undeniable fact when the Gestalt and psychoanalytical schools appeared in full force in the United States, under circumstances that I do not need to describe. The behavioristic camp assumed what might be called an aggressive defensiveness. Moreover, although the existence of well-intentioned attempts at understanding cannot be denied, the pattern of a disorganizing defensiveness dominated the impulses to co-ordinate the field and to establish denominating fundamentals that could become the basic structure of a total field of psychology. Attempts like that made by Coleman R. Griffith in his *Systematic Psychology* were rare.

Once, in the late 1930s after I had returned from an Annual Meeting of the American Psychological Association that had given me much to think about concerning what I have just described, I met James McKeen Cattell, whose frank honesty in scientific matters I had always admired. I reported to him the impression I had gained from the meeting, and he told me that he himself was so discouraged about the status of psychology that he had given up attending any kind of psychologists' gathering. With his plastic, ironic humor, Cattell remarked: "You know, psychology looks to me like a gigantic, monstrous jellyfish, a creature without bones. What concerns me is that the entire field ought to be burned over like a blueberry hill." The proposal of a psychological blueberry burning did not register with me positively. However the comparison with the boneless jellyfish impressed itself unforgettably on my mind.

It would not take much effort to impress upon anyone the enormous growth of psychological research publishing. *Psychological Abstracts* that in 1925 listed 2,500 titles has in the last three years approached 10,000 titles annually. This material of course can be classified, and it is between the covers of the *Abstracts* or in some other way. Still there is this tragic lack of co-ordination, integration, and, even more, a lack of basic perspective, denominating funda-

mentals, and the suppositions necessary to organize the field into an organic whole.

I know that I have been labeled a dreamer about unimportant generalizations. Nevertheless science does not grow by an amassing of details alone; more important is a careful inductive or deductive development of theoretical aspects and even their co-ordination. Remember that with the awakening of consciousness at the dawn of this century, the enormous growth of the scientific world made imperative the need for reorientation of insights into the very nature of the organ by which they were created, the inner and personal nature of man. The scientific field upon which this task should devolve—psychology—precociously and proudly announced that it was destined to become the basic and fundamental science. I ask, how can a scientific field soundly assume such a role while it remains an ever-increasing, mountainous, heap of unrelated and unco-ordinated facts?

I hope I am not misunderstood in this. I am of course not against the great interest in and great effort toward individual advance in special problems related to or included in the field of psychology. I am also not opposed to the multiplicity of aspects, theories, and terminology. However what I find lamentable is the lack of an effort to work these scientific raw materials into the finished product of a science that then, and not until then, will be able to determine for itself not only its own autonomy but the factors by which it may successfully co-ordinate other fields, or even become a fundamental science. Of course, we have all these applicative attempts. However we know too well how much more dissatisfaction and how, in many cases, more confusion is created than help given or satisfaction harvested.

Sincere people have even spoken of a disintegration of psychology, and many are turning away disillusioned regarding the help that their scientific fields was promised by psychology.

That something should be done about what I call the Dilemma of Psychology is the motive behind the assembling of these papers. The purpose of this monograph is not only to make available the remarks of some distinguished authorities about what everyone will recognize as a basic subject in psychology, but to inspire a great deal of basic thinking and work in the field, to achieve what James McKeen Cattell called "getting some bones into the jellyfish" of psychology. It is hoped that our conference that led to this monograph will be the first of an annual event, an annual conference on a basic major problem of psychology that I frequently have designated psychological fundamentalism. Also I hope that all workers in this area will help to transform this hope into an attempt to turn back the forces of disintegration and to work for psychology as a soundly established scientific field.

BASIC CONCEPTUAL PROBLEMS IN THE PSYCHOLOGY OF THINKING

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Introduction

Of all the baffling problems that have fallen to the lot of the psychologist, those pertaining to human thinking have been the most frustrating. Nevertheless, no psychological problems are more important. The critical, general human problems presented by the world in which we find ourselves call for the exercise of thinking as never before. If we are to make the most of our intellectual resources to meet the challenges that daily come our way, it is imperative that we understand more about the processes of human thinking. This monograph is a tangible recognition of the need for concerted effort to gain the required understanding.

Some pioneer attempts. The history of psychology exhibits a number of abortive attempts to reach a more significant and useful comprehension of thinking. William Wundt is said (Boring, 1950) to have thrown up his hands, recognizing the inadequacy of his analytical, introspective method to cope with the problem. Titchener and O. Kuelpe refused to accept the limitations of that approach (Titchener, 1909). Titchener, however, contributed practically nothing by attempting to account for thinking in terms of sensations and images. Kuelpe and his students fared somewhat better when they sought descriptions in other kinds of terms, but they were up against the fact that most of thinking is well beyond direct observation. The glimpses that they caught did yield a few reasonably descriptive concepts. However, we may truthfully say that they hardly scratched the surface of the problem.

Watson (1929) also failed when he attempted to catch the essence of thinking behavior in terms of speech reactions and other muscular movements. The difficulty with a purely peripheral approach to thinking is that the phenomenon is essentially central. Although we can learn much regarding thinking processes by the study of stimulating conditions, as later discussion will show, the motor reactions of a thinking individual seem to bear very little correlation with thinking as such, at least in unique ways that enable us to infer much regarding thinking operations from muscular contractions. As my teacher in philosophy used to say, we have to *stop* to think and this means immobility rather than muscular contractions.

Concepts for describing thinking. In spite of promising beginnings in the more recent experimental approaches to thinking, it has seemed to me that one of the critical needs has been for new and more functional concepts. Concepts are tools of research. Full comprehension of the phenomena in a domain of investigation requires answers to the questions of *how* and *why*. However we cannot make substantial progress toward answering these major questions without having first answered the question *what*.

What are the phenomena that exist in the domain of human thinking? What are the different kinds of operations or processes? Much experimenta-

tion has proceeded on the basis that we know what the fundamental kinds of operations are, too often without questioning whether we could not have better conceptions. There is even inadequate definition of the operations that are assumed to exist, for example, that most frequently mentioned term "problem solving." What is the unique referent for "problem solving?" No one can answer this question. Furthermore, psychologists have all too often fallen victim of the fallacy of assuming that one name means one process. They have also not demanded empirical concepts, which are essential for unambiguous communication.

Some of our working concepts have come to us as a part of our philosophical heritage; such terms, for example, as reasoning, abstraction, induction, deduction, and imagination. All of these concepts suffer from the difficulties just mentioned. Of somewhat better status are concepts arising rather incidentally in connection with experimental investigations. Of Würzburg origin are the concepts of *Aufgabe*, set, determining tendency, and schema. From scattered sources we have concepts such as vicarious trial and error, insight, search model, and functional fixedness. Some of these terms have clear empirical reference but some of them, such as "insight," do not. At any rate, the paucity of concepts of the better kinds is a serious handicap and the few concepts that we have cannot support fully fruitful investigations of problems of thinking. In addition to their shortness of number, there is the further weakness that they do not fit into any unified theory of thinking.

It is the major purpose of this paper to suggest a quite different approach, one that is uniquely suited to the generation of concepts and that answers the question of *what* thinking operations exist. The concepts arising from this approach are clearly empirically based and also fit into a unified theory.

An Approach through Individual Differences

The approach in question is an experimental application of the logic and the operations of factor analysis. In the past, factor analysis has been generally employed by those who are interested in problems of personality, that is to say, in problems of individual differences, and especially by those with some practical interest in vocational problems. It has not been commonly recognized that factor analysis has potential value for contributing to knowledge of basic psychological facts and to general psychological theory. Only recently has the method demonstrated that it has much to offer in this direction.

Two general approaches to psychological research. Experimental psychology during most of this century has operated in terms of a basic model of the living organism that has become so much second nature to psychologists that it is very rarely questioned. It is a stimulus-response model. Observed relationships have been restricted very much to correlations of responses with stimuli. Laws are stated in the same terms. Following the pattern of analysis developed in the physical sciences, experimental psychologists have attempted to control all independent variables except one at a time.

The factor-analytic approach differs from this way of thinking and experimenting in two significant ways. On the one hand, instead of observing stimulus-response relationships, it examines the covariations among items of response information. This is done with the conviction that where individuals of a

group show relatively consistent concomitant variations in behavior, in response to standardized situations, there is an underlying disposition or trait determining that phenomenon. The nature of the underlying trait is inferred from the observable common features in the behavior.

The other major difference between the stimulus-response and the factor approach is that where the former favors the univariate experiment the latter capitalizes upon a multivariate experimental design. In dealing with behavior, an advantage of the latter is its adaptability for coping with the enormous complexity of the phenomena to be described and understood. It takes individuals as they come (within a specified group) and, for the immediate purposes of the analysis, it need not be concerned with the way in which the individuals became what they are. Experimental psychology should be concerned with this question, but apart from carefully nurtured, inbred colonies of rats it can rarely control prior development and does not do so. In the multivariate approach, within broad limits, such genetic variables can be ignored. This is not to say that genetic problems are unimportant. In fact, it may also be said that genetic psychological problems, too, can be more meaningfully attacked if we already know the *whats* of behavior.

The logical integration of the findings of factor analysis with those of stimulus-response psychology is not difficult. Whereas factor analysis comes out with traits, which describe how individuals differ, the study of individuals deals with what may be called functions, which describe how individuals are alike. Discovering in what ways individuals differ also reveals the ways in which they are alike. Hence, with the appropriate use of factor analysis, we learn how the typical individual functions. The term appropriate should be emphasized, because factor analysis is not always applied in a way that will yield information concerning basic psychological variables.

A factor-analytic experiment. A good factor-analytical experiment whose objective is to learn something of basic psychological significance observes several requirements. First, the investigator begins with hypotheses. In some area of behavior, such as that of visual perception for example, he might hypothesize that seeing visual depth is a function separate and distinct from all other visual-perceptual functions. According to the hypothesis, individuals should be expected to differ from one another in ability to deal with tasks involving depth perception.

The investigator then sets about developing three or more tests, each of which he thinks should indicate such individual differences and each of which is sufficiently different from others in this group of tests to justify believing that they are not just alternate forms of the same test. At this stage, he has no basis for knowing whether all of the tests do indeed indicate individual differences in the same attribute and, if they do, to what extent they succeed. The investigator will think of other perceptual functions that he thinks are distinct from depth perception and from one another and will develop a few tests for each additional hypothesized factor. He will expect the pattern of intercorrelations among all the tests so developed to tell him, through the operations of factor analysis, which of his hypotheses are supported and which are not.

Conditions controlled in a factor-analytical experiment pertain to the tests,

the circumstances under which the tests are administered, and the selection of experimental subjects. The population of individuals sampled should be relatively homogeneous with respect to any incidental conditions that have bearings upon the intercorrelations. For example, it is often desirable to control for age, sex, and educational level. As much as possible, we want the intercorrelations to be determined by the common factors only. The tests themselves should help to control the behavior of examinees so that as much as possible the differences in scores on a test reflect only differences in common factors. The test instructions are designed further to exert some degree of control to help standardize what the examinees do. Attention is also given to the motivational and emotional conditions of the examinees and to the external, environmental testing conditions.

Conditions systematically varied pertain to the tests given. The variations may be qualitative, involving differences in the kinds of tests included in the test battery, or they may be quantitative. In the latter case, continuous variations may be introduced in the form of timing, difficulty, complexity, or degree of restriction of certain kinds. Such variations would be introduced to test hypotheses concerning the optimal kinds of tests for the factors.

The nature of a factor is inferred from the properties that its tests have in common. The tests, their material, and their demands upon the examinee serve as the experiential referents for the factor concept. Theoretical concepts built upon the basis of factors also share the same empirical bases.

A Theory of Intellect

"Ability to think" is often given as a definition of intelligence. As I shall show, intelligence is much more than that. However it is my belief that we can have a much clearer conception of the nature and the boundaries of the concept of thinking if we consider its relations with intelligence in general. Accumulation of results during the past 25 years have made possible the development of a unified theory of intellect, which includes thinking. That theory will be very briefly explained and its implications for general psychological theory, particularly for theory of thinking, will be pointed out.

The parametric model of intellect. As of this date, about 55 factors that may be regarded as aspects of human intellect have been discovered.* Consideration of the properties of these factors has led to a systematic arrangement of them within a parametric model (Guilford, 1959). The factors differ in at least three major ways: in the kind of operation involved in the task, the kind of material or content, and the kind of product or outcome. FIGURE 1 shows these variations in terms of a three-dimensional model. The kinds of operations, contents, and products that have been recognized are shown.

The categories of products are perhaps the most interesting and revealing. They represent the forms in which the individual casts his information, regardless of the kind of content, and also the forms in which information is used. Units of information are relatively segregated items with "thing" character. Classes are sets of items of information the members of which have certain

* About half of these can be credited to the work of the Aptitudes Project at the University of Southern California during the past ten years, under Contract Nonr 228(20) with the Office of Naval Research, Personnel and Training Branch. The ideas expressed in this paper are my own and are not necessarily endorsed by the sponsoring organization.

common properties. Relations are various kinds of connections between items. Systems are organized groups of interacting items. Transformations are changes, shifts, or reinterpretations. Implications are extrapolations of information to antecedents, concomitants, and consequents.

The general psychological theory implied. An important generalization to be extracted from these definitions is the paramount role of the concept of information. In this connection, information is defined as that which the individ-

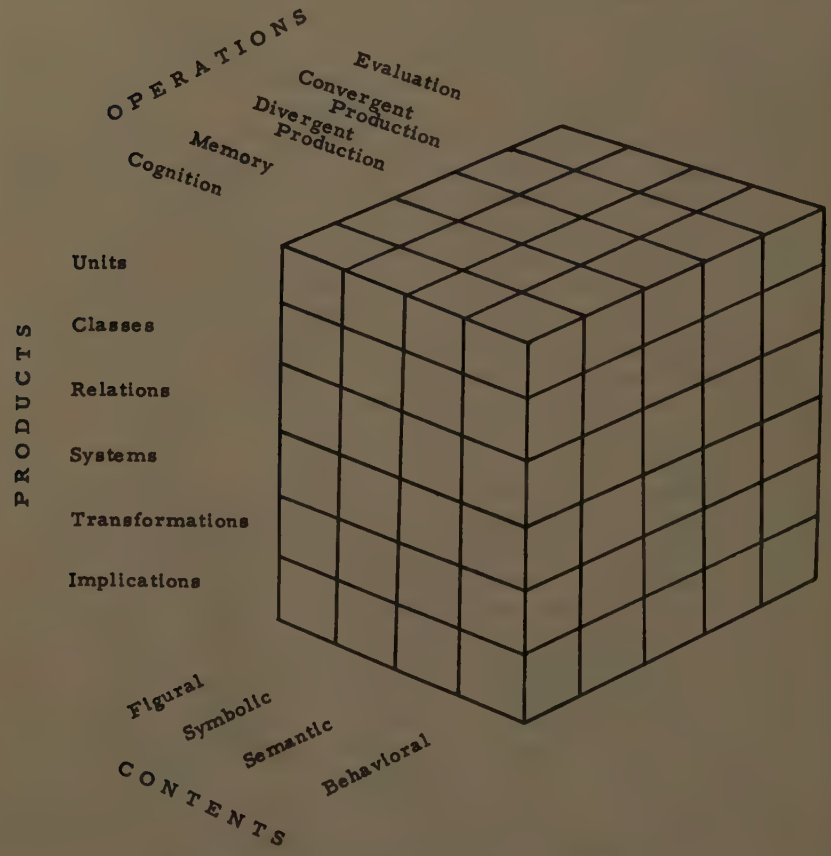

FIGURE 1. Model for the structure of intellect.

ual discriminates. This can be regarded as an empirical concept, for we can infer whether an individual has a discrimination by virtue of his differential reactions. There is no implication that the discrimination has to be conscious or that conscious experience enters into making the discrimination. Nevertheless it can be suggested that conscious phenomena came into existence because they extend enormously the potentialities for making discriminations.

The categories given along the dimensions of the model can be briefly defined. The cognitive operation provides discovery, comprehension, or understanding, in other words, the awareness of information. The memory operation is the

storage of information. The two production categories involve the generation of information from given information. In the divergent form of production, a variety of answers is produced; in the convergent form, unique or rather fully determined answers are produced. Evaluation means decisions as to whether information that is known, remembered, or produced meets certain standards or goals.

The categories of content are what might be called the broad varieties of information. Abilities to deal with concrete (perceived) information are put into the category called "figural." Two content categories deal with abstract material: symbolic and semantic. Symbolic material is in the form of letters or numbers and the like. Semantic material is meaningful or verbal-conceptual. The category of "behavioral" has been added on a purely theoretical basis, no such factors as yet being known. It covers what has been called "social intelligence." The phenomenon often called "empathy" probably belongs in the area of cognition of behavioral content.

With information as a key concept, we are led to a rather different view of the living organism. The traditional stimulus-response model gives way to a more comprehensive one. From the psychological point of view, the organism is an agency that deals with information. Information comes to him in terms of the four content categories: figural, symbolic, semantic, and behavioral.* He deals with it in terms of the various kinds of products.

The listing of the six kinds of products probably does not do justice to actual dealings with information. It is quite obvious that two or more products may be involved in any act of the individual, but there are other kinds of interaction possible between products. It might be thought at first, as we did, that only units would be involved in connection with other products, for without units, nothing; units are basic. Classes and systems can obviously be formed of units. Relations and implications exist between units, and transformations begin and end with units.

However, recent evidence shows that we can have classes of relations and classes of systems as well as classes of units (Guilford *et al.*, 1960). Presumably there are also classes of transformations and implications. Analogously, we may hypothesize that there are systems of relations and transformations, as well as of classes. Such interactions extend very greatly the possibility of description of complex mental activity.

While we are on the subject of general psychological theory, it may be pointed out that this informational point of view automatically takes sides on several perennial issues. Although it offers an entirely objective approach to psychology, it has some linkages with prebehavioristic concepts, giving those concepts new significance. It makes possible a central psychology, rather than an almost purely peripheral one, leading to a rich vocabulary of intervening variables.

This theory goes well beyond the time-honored explanatory concept of association. The concept of association has been eminently useful, but reliance completely on it has put considerable restriction upon our science, especially

* Factor-analytical results have suggested that the figural category subdivides along the lines of sense modalities: visual, auditory, and kinesthetic, at least. The behavioral category may subdivide depending upon whether the information refers to one's self or to others.

when association is further limited to stimulus-response connections. The products of information just mentioned are not best interpreted as varieties of association (some of them, indeed, cannot be so interpreted) but rather in accordance with the thinking of the psychology of *Gestalt*, without necessarily adopting all of the *Gestalt* principles (Guilford, 1960).

Traditional Concepts and the Structure of Intellect

Some of the traditional concepts pertaining to thinking and related subjects find very clear, localized places within the structure-of-intellect model. Cognition, memory, and evaluation (which is a synonym for judgment) are represented by respective operations categories. Productive thinking is represented by two operations categories—divergent and convergent production—that bring out a distinction that promises to be very useful. It could he said that the category of evaluation also comes under thinking, representing the concept of "critical" thinking.

Some localized concepts. Some of the more limited concepts connected with thinking can be assigned to localized regions of the model of intellect but some cannot. Let us consider first a few that can be localized, first of all the concept of "concept." Concepts imply classes, and this immediately suggests the product of classes, represented by a complete layer in the structure of intellect. For the most part, tests of cognition of classes have called for recognition of familiar classes. It is probable that, in such tests, the examinee is not required to form new and unfamiliar classes. In tests of the divergent production of classes, the examinee is given a list of units and is asked to form as many different classes as he can, using each unit as many times as he wishes. Tests of the convergent production of classes are similar except that only one set of classes is possible. It is not clear, as yet, which of the five operations categories is most involved in experiments on concept formation; any or all of them might be involved.

Induction and deduction have been traditional varieties of thinking. In my opinion, they have never been satisfactorily defined as psychological concepts. Induction has been characterized chiefly as a way of going beyond particular experience to the knowing of something general or abstract. "Knowing something general or abstract" suggests cognition, hence we may localize induction in the operation category of cognition. If we ask which of the products represent knowing something beyond the given, something with general significance, it would appear that classes, relations, and systems qualify.

Classes, relations, and systems are mental constructs that have transposability to different sets of units. It could be argued that transformations and implications are of a similar nature, but this is not so clear. We may say, then, that induction is defined in terms of the structure of intellect as including the cognition of classes, relations, and systems for all kinds of content. To distinguish these three subcategories as varieties of induction, we might call them classificatory induction, relational induction, and systemic induction.

Deduction is roughly defined as the drawing of conclusions from given information. There is usually the implication that the conclusions drawn are strongly, if not entirely, determined by the given information. Drawing con-

clusions is an act of productive thinking. The highly determined response suggests the convergent-production category.

Of the products to which the term deduction applies, only two seem to qualify. One is the convergent production of correlates. A correlate, following C. S. Spearman's terminology, is a unit of information that is needed to complete a relationship where the relation and one unit are given. The reason for regarding such a response as a kind of conclusion is that the production of a correlate is the final step in reasoning by analogy. We may speak of this kind of conclusion-drawing as relational deduction.

The other kind of deduction pertains to the convergent production of implications. Given two or more items of information, another item follows as day follows night. We may call this implicational deduction. It comes closest to what has usually been regarded as deduction in general. There can be some argument, therefore, concerning the extension of the term deduction to include the production of correlates. There are better arguments for not extending the concept beyond the two products of relations and implications.

The concept of rigidity in thinking has been illustrated by the discovery of factors of flexibility (Frick *et al.*, 1959). Three kinds of flexibility have been found in connection with thinking. One of these has been called "spontaneous flexibility" for the reason that it came out in certain tests in which there was no apparent necessity (apparent to the examinee, that is) for being flexible. A typical score came from the Brick Uses test, in which the examinee is told merely to list all the uses he can think of for a common brick. The flexibility score is the number of kinds of uses, not the total number of uses. It was eventually realized that kinds of uses represent classes and that the ability may be regarded as the ability to go readily from one class to another. The person with a low score for this kind of flexibility perseveres within the same class. This is a special kind of rigidity.

Another kind of flexibility has been called adaptive, for the reason that, in some tests at least, the examinee is forced to change his direction of thinking or his strategy in working on an item if he is to make a good score. It was eventually realized that the changes can be interpreted as transformations, which involve reinterpretations. More than one change may be necessary, as for example in a test in which the examinee is told to solve the same item in different ways. The kind of rigidity involved, conversely, is a tendency to keep going in the same direction or to be inhibited from making transformations. Both kinds of flexibility factors are in the divergent-production category, and both pertain to all kinds of content so far as we know.

It has been recognized that a third kind of factor accounts for a type of rigidity known as functional fixedness. A classical experiment on this phenomenon has presented the problem of tying two ropes together when the two hang from the ceiling so far apart that the subject cannot simultaneously grasp both of them. Incidental objects lie on a table, among which one or two could serve as aids in the solution. Some subjects pick up the pliers, tie them to the end of one rope, and swing the combination like a pendulum. With the free rope in one hand, the successful subject catches the other on its approach swing.

One of the pencil-and-paper tests used in our Aptitudes Project is called "*Gestalt* Transformation" and it presents items such as the following:

From which object could you most likely make a needle? (1) onion, (2) splice, (3) cabbage, (4) fish, and (5) steak. The keyed answer is "fish," with the thought that a bone from a fish, given an eye, would be most readily adaptable. This test has consistently helped to identify the factor of redefinition. It is believed that those who do well in such a test can redefine or reinterpret objects in order to adapt them to specified new uses. Such a test seems clearly similar to the pendulum problem just described.

We also find parallel redefinition abilities, one pertaining to symbolic content (words that must be reorganized) on the one hand, and figural content (complex figures whose lines must be reused to form simpler, hidden figures) on the other. All these factors are in the transformations row of the convergent-production category.

Reasoning and problem solving. A number of analyses of reasoning tests of various kinds have been made, leading to a number of factors that are not very systematically placed within the model of intellect (Green *et al.*, 1953; Guilford *et al.*, 1954). The factors already discussed in connection with induction and deduction account for most of them, but it is very questionable whether all the induction factors would be recognized as reasoning abilities. It is probably best to conclude that reasoning is so poorly defined psychologically that it cannot be given a unique status in the structure of intellect.

The same can be said for the concept of problem solving. However, because of the great interest in problem solving as a subject of psychological study, it is worth our while to consider what can be made of it in terms of the factorial system. What we conclude with regard to problem solving in this respect also applies in part to reasoning.

As I have suggested elsewhere (Guilford, 1954), problems are extremely varied and their solutions involve different operations, contents, and products, depending upon the circumstances. There is no convincing evidence that I know of to the effect that a unitary, blanket problem-solving ability has been demonstrated. From the factor-analytical point of view, then, the investigator's task is to attempt to specify the pattern of abilities that contribute differentially to the solution of each type of problem. We and others have done this kind of analysis, more often incidentally than intentionally. I shall cite some examples of analyzed tests.

One of the incidental findings pertains to problems of the type found in a common arithmetical-reasoning test. Repeatedly, analysis of such a test reveals the differential operation of two intellectual factors plus minor contributions of a few other factors. The leading component of the total variance can be attributed to the factor of general reasoning. This factor commonly accounts for about 25 percent of the total variance (Guilford and Lacey, 1947). It has been identified as an ability to structure or to comprehend problems preparatory to solving them (Kettner *et al.*, 1956). It is probably a more general ability to cognize semantic systems and is placed accordingly in the model of intellect.

The second prominent factor in arithmetical-reasoning tests is numerical facility, the ability to do numerical operations, that contributes about 20 percent of the total variance. Other small contributors, usually to the extent of less than 10 percent each, are verbal comprehension, spatial visualization, and

spatial orientation. An arithmetical problem is stated in verbal terms, hence the comprehension of word meanings may make some small differences in scores. Some problems either pertain to spatial arrangements involving distances and directions or the examinee may think of the problem in spatial terms, hence the variances in the two spatial factors. Spatial-orientation ability pertains to a static appreciation of spatial arrangements (cognition of figural systems) whereas spatial-visualization ability pertains to changes or movements in space (cognition of figural transformations).

A few other factors are usually present in an analysis in negligible amounts, but there is an appreciable amount of predictable or true variance unaccounted for. One or two incidental analyses suggest that, within the true variance still unaccounted for, there may be appreciable components attributable to the

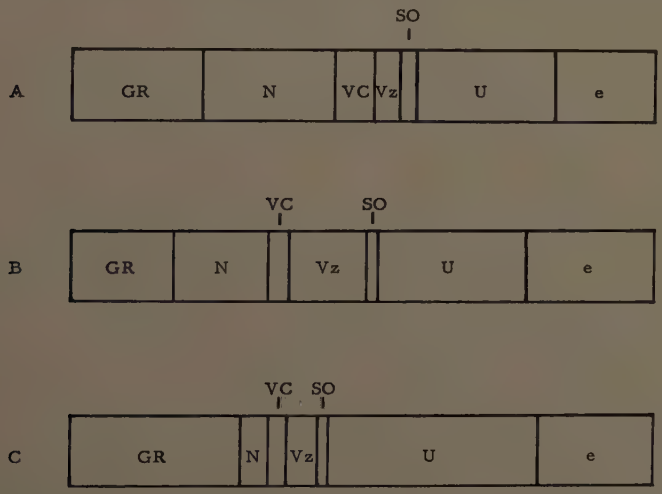

FIGURE 2. Proportions of variance in two analyses of the same arithmetical-reasoning test (A and B) and one analysis of a second test (C). Code to the symbols: GR = general-reasoning factor; N = numerical facility; VC = verbal comprehension; Vz = visualization; SO = spatial orientation; U = unknown true variance; and e = error variance.

factor having to do with the cognition of semantic relations, the cognition of symbolic systems, and the evaluation of semantic relationships (logical evaluation). It is especially noteworthy that most of all the factors involved are in the cognition category. Understanding the problem, in this case, appears to be much more than half the battle.

FIGURE 2 is presented to illustrate some of these points, plus others. Diagrams A and B represent the results of analysis of the same form of arithmetical-reasoning test in two different samples to show how much consistency may be expected in a study of the factorial composition of a test. Such consistency is usually obtainable if the samples are from the same population, the two test batteries analyzed are much the same, and the samples number at least 200. Diagram C represents the results of analysis of another form of test (Guilford and Zimmerman, 1956), which had been constructed with the aim of minimizing the variance in numerical facility, apparently with much success. The

increase in variance in spatial visualization can be attributed to the larger proportion of problems that were presented in pictorial form. The changes in relative weights of the factors demonstrate how minor changes in tests of the same name and character may affect the demands that the tests may make upon human resources. The larger proportion of unknown variance in the second test form (diagram C) reflects the fact that the analyzed battery was small and only the five factors mentioned were accounted for.

In a study recently reported from our Aptitudes Project, we had the special goal of accounting for individual differences in solving certain types of problems in terms of factors of intellect (Merrifield *et al.*, 1960). Two of the problems call for the filling of gaps between initial and terminal information. In Missing Links, an initial word and a terminal word are given, the examinee to insert three words that make a reasonable chain of associations. For example, given "RED ——— ——— ——— BEER," the examinee might insert the words "sunset, weather, and cold."

A test called Transitions presents items in each of which the beginning and end of a short story are given, the examinee to fill in the intervening gap with reasonable events. The Predicaments test presents in each item a not uncommon kind of practical difficulty. For example, a group going on a picnic takes the materials from which to make sandwiches but finds that the cheese has not been sliced and that no one has a knife. There are available four objects: harmonica, matches, thermos bottle, and ukelele. The examinee is to tell two ways of using one or more of these objects to slice the cheese.

The bar diagrams in FIGURE 3 indicate the proportions of the common-factor variance for each kind of problem that can be attributed to each common factor, where there is enough to show separately. The OC segment in each case stands for "other common factors" too weak to be mentioned separately. The starred factors were represented in their tests to degrees regarded as "significant" (loadings of 0.30 or higher, or proportions of total test variance of 0.09 or greater). The proportions of total-score variances accounted for in each test were 0.34, 0.41, and 0.36, respectively. The proportions of total variances that could be accounted for, as estimated by the reliability coefficients, were 0.58, 0.55, and 0.46, respectively. Thus, in this analysis, from 60 to 80 percent of the variances that could be accounted for was accounted for. I mention all these figures not because I believe that they are very precise estimates but to show the kind of results that are attainable by this approach. It would take very large samples to determine all such estimates accurately. Our sample numbered 219.

One or two things are noteworthy regarding the bars in FIGURE 3. A common factor that is involved in all three kinds of problems is our old friend verbal comprehension (VC). It is distinctly more prominently involved in Missing Links and Transitions than in Predicaments, a result that is very reasonable. Forming chains of associations in Missing Links requires much familiarity with word meanings. Writing connected and coherent discourse in Transitions has a similar requirement.

All three tests involve the factor called sensitivity to problems (SP), although significantly so only in one of them. This may seem strange in that the problems are presented to the examinees; the examinees do not have to become

aware on their own that such problems exist. There are two possible rationalizations. On the one hand, it may be that in the solution of the problems, it pays the examinee to note that subproblems exist. On the other hand, the factor is interpreted as the evaluation of semantic implications. Put in this light, we may say that the problem-solving processes in these tests involve implications that must be evaluated.

The factor of general reasoning (GR) is involved in two of the tests, significantly in one. Although the problems are presented rather fully, there is evidently something left for the examinee to do in the way of structuring or cognizing systems. If a story plot is a system, however, it is curious that

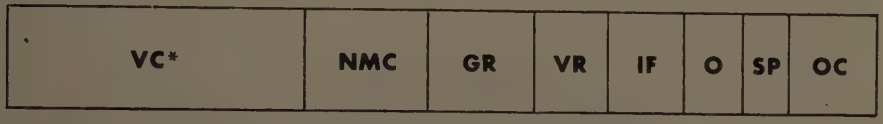

FIGURE 3. Examples of analysis of common-factor variance in problem-solving tasks into proportions accounted for. The starred contributions can be regarded as significant. Code: VC = verbal comprehension; CF = conceptual foresight; GR = general reasoning; NMC = convergent production of semantic classes; VR = cognition of verbal relations; IF = ideational fluency; SP = sensitivity to problems; O = originality; and OC = other common factors combined.

factor GR is not involved appreciably in Transitions. A possibility is that the kind of system in this test is behavioral rather than semantic, which would call for a parallel but separate ability.

The factor of originality (O) is involved significantly in Transitions and very slightly in Missing Links. Originality is now interpreted as a matter of divergent production of semantic transformations involving reinterpretations of meanings in order to produce a variety of responses. It is not obvious just where such reinterpretations occur in a somewhat complex task like the Transitions test, but with a result like that pictured for this test the procedures by which individuals solve the items should be examined in an attempt to pinpoint its involvement. In the Missing Links test, the examinee may have to reinterpret some words in order to make them fit into the chain of associations.

May I mention only incidentally a point regarding methodology. Some

investigators maintain that the best way to understand problem solving is to give their subjects very complicated problems, tasks that may require as much as three hours time. I think that for most purposes this is definitely faulty technique and quite unnecessary. The more complicated the task and the longer the time the subject has to work on it, the more we lose experimental control and the less we know about the results. Scores for such problems have very low reliability, typically in the neighborhood of .25 (Chorness, 1959), hence there is little accuracy of measurement and little predictable variance.

Another practice that is experimentally bad from the standpoint of ambiguity is to give a test of problems, each item of which is of a somewhat different kind. The factor composition thus probably changes from item to item. We know much better what we are doing and we also control better what the subjects are doing if each test is relatively simple and homogeneous. We can gain information about the nature of the relatively more complex problem-solving activities by varying systematically the simple tests that we correlate with them. It is best to vary kinds of items between tests, not within tests.

Creative thinking and planning. Until recently, creative thinking has been a rarely used concept in psychology, and planning has had no particular status other than in popular usage. Planning partakes of some of the properties of problem solving, for a plan is often a means of solving a problem. Planning also suggests creative thinking because of its inventive aspects. We may well consider these two concepts together.

The general area that can be considered creative thinking has been explored by factorial methods, with some results that are very enlightening. One of the initial expectations in our Project's consideration of the subject pertained to fluency of thinking, the speed or facility with which an individual can produce ideas (Guilford, 1950). Another expectation emphasized flexibility of thinking or freedom from rigidity. Still another emphasized originality, the ability to produce novel ideas. The results have supported these expectations rather well (Wilson *et al.*, 1954; Kettner *et al.*, 1959).

Some of the fluency factors pertain to the speed of calling up or producing units of information, some of them pertain to the production of a variety of correlates, and still others pertain to facility in producing systems (Guilford and Christensen, 1956). The emphasis is on the plural, for multiple responses and variety are special features of tests of fluency.

Reference was made above to the two kinds of flexibility factors: spontaneous and adaptive, the former pertaining to the production of a variety of classes and the latter to the production of a variety of transformations. Reference was also made to another type of freedom from rigidity in the form of redefinition factors.

Some of our initial expectations were not fulfilled. Our studies have not shown that the expected abilities to analyze and to synthesize are unitary traits (Wilson *et al.*, 1954). The failure to find them as unities does not mean that we do not analyze or that we do not synthesize. Analyzing and synthesizing activities are probably to be accounted for in terms of the intellectual factors but, as in the case of problem solving, the factors involved will depend upon the circumstances. What is commonly called analyzing a problem, for ex-

ample, may actually be the cognition of a relationship, a system, or an implication.

One kind of factor that should be classified with the other creative-thinking abilities was found in connection with our study of planning abilities (Berger *et al.*, 1957). It was hypothesized that a planner characteristically elaborates upon ideas if he is to make a plan complete in every detail. A factor that could be identified as an elaborative ability was discovered, and it has been interpreted as the ability to produce a variety of implications. We have analyzed a test calling for the production of a somewhat elaborate plan to cope with a morale problem on a military base. Individual differences in success in this test were accounted for in part by the factors of elaboration, originality, and ideational fluency, along with other common factors, each to smaller extents.

The various factors of fluency, flexibility, and elaboration constitute the category of divergent production, a kind of productive thinking that goes searching or that takes different directions, the end result being a variety of outcomes. It has therefore been tempting to identify the concept of creative thinking with the divergent-production operation category.

This identification could be defended, but it would overlook some other factors that seem relevant to creative production. For example, the redefinition factors representing (in reverse) a form of rigidity are placed in the convergent-production category. It has been thought that the factor of sensitivity to problems is of considerable importance to creative people, yet it is in the evaluation category. Furthermore, if we consider the great variety of things that creative performances entail in many fields such as theory construction, invention, composing, and writing, we have to recognize that many of the other intellectual abilities may also play roles. We may conclude that, although the more crucial abilities for creative thinking are in the divergent-production category and also perhaps in the transformation layer of the model, in other respects the contributors to this kind of activity are scattered as in the case of problem solving.

Future concepts pertaining to thinking. The concepts of thinking that have been generated in connection with the structure of intellect have much clearer empirical references than the traditional concepts; they are more comprehensive, as well as more analytical, and they exist in a logical, unitary system that also relates them to cognition and memory. It might therefore be proposed that they replace the traditional concepts. There is much value, however, in keeping ties with the past, and, as I have shown, it is possible to apply certain traditional concepts to blocks of factors within the model of intellect. These alignments help to enrich the nature of the traditional concepts and to sharpen their boundaries. We also have to recognize that old concepts do not die quickly; to apply a military analogy, they tend to "fade away."

There is no implication in what I have said to the effect that the new concepts will necessarily be adequate for all time. Conceptions in science must be flexible and we must always be open to improvements and additions. The test of the value of new concepts lies in their power to supply fruitful discriminations.

The progress of psychology, like that of any science, depends upon more

and more penetrating analyses. I regret to say that many psychologists still seem to be afraid of analysis. Some others, who are less allergic to analysis, happen to take an analytical approach that is not so very fruitful. The type of analysis that we choose should be tailored to the needs of the problems as we see them. I do not maintain that the sole or best approach is that of factor analysis. Other kinds of analysis are also needed. As long as we approach problems analytically we have the chance of discovering something. If the results are disappointing, the reaction should not be that of turning our backs on analysis. The cure is to be found in further analyses.

Summary

Much of the difficulty encountered in the experimental investigation of thinking can be attributed to a critical deficiency of analytical, functional concepts. We cannot very well discover how we think or why until we know what the basic processes of thinking are.

Unique thinking abilities found by factor analysis can also be regarded as ways of functioning of the individual. They should therefore provide useful concepts for the study of thinking. Such concepts have the virtues of being empirical, analytical, and comprehensive, and they fit into a unified theory of intellect, which relates the thinking concepts to one another and to operations of cognition and memory.

The system emphasizes kinds of information, kinds of operations, and kinds of products; it emphasizes what an organism does with information. The traditional concepts of cognition, memory, thinking, and judgment are represented as major kinds of operations. Thinking is accounted for by the two major categories of divergent production and convergent production and the category of evaluation.

Certain traditional concepts find clear places within the system and thus acquire the status of being empirical as well as systematic. Three varieties of induction are recognized within the cognition category and two varieties of deduction within the convergent-production category. Three varieties of rigidity (or flexibility) are recognized, two of them in the divergent-production category and the third having to do with transformations in the convergent-production category. Other concepts that have not been successfully localized within the system are reasoning, analysis, and synthesis. This is probably due to the amorphous nature of those concepts.

Other concepts of a broader nature, such as problem solving, creative thinking, and planning, being less standardized activities, have more widespread and relatively inconstant affiliations within the system. Probably the abilities most crucial for creative thinking are in the divergent-production and transformations categories. However, problem solving particularly being almost as broad a phenomenon as behavior itself, seems potentially related to a great number of intellectual abilities depending upon the circumstances. It is possible by factor-analytical procedures to determine the differential roles of the various intellectual functions in each complex thinking activity as carried out by each kind of population.

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INDIVIDUAL AND COLLECTIVE PROBLEMS IN THE STUDY OF THINKING

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A Dutch colleague has done us the service of classifying and counting the articles appearing between 1950 and 1954 in the *Psychological Abstracts* under the heading of "Cognition" (van de Geer, 1957). Of the total in this category (a rather staggering total of about 4471 titles) nearly one third deal with learning and memory and nearly two thirds with perception; the remainder, about 6 per cent, concern themselves with the topics of thinking and imagination. I have no data for the six years since then, but I have the strong impression of a most vigorous and disciplined renewal of activity in the field.

There have been interesting signs. Sir Frederic Bartlett (1958) has published a major book on thinking, likening it to the symbolized internalization of motor skills. Several major books from the Geneva laboratory have been written and translated into English in the last few years, perhaps the most important of which is the very challenging *The Growth of Logical Thinking* (1958) in which Bärbel Inhelder and Jean Piaget take the child to the brink of adulthood at which point concrete operations are replaced by the propositional or formal operations we know as adult thought. Luria (1959) in the Soviet Union has carried ahead the tradition established by Vigotsky's work and has begun to show the manner in which language and symbolizing serve to free the organism from the control of stimuli so that it may gain control over its own activities through the transformations of thought.

At the Cognition Project at Harvard, my associates and I have shown the way in which aspects of thinking can be conceived of as planful strategies designed to gain and organize information while at the same time regulating the risks of failure and the strains of overload brought about by man's highly limited capacity for processing information at any given moment of time.

The field of psycholinguistics has flourished as a result of the influences of linguistics and of information theory. At last we have stopped treating Benjamin Whorf (1956) as a delightful oddity whose claims were the intriguing stuff we could put into introductory lectures while assuring our colleagues that his concept of linguistic relativity was patently absurd. We have learned about the tremendous power of the discontinuous variables of structural linguistics: phonemes, allophones, lexemes, distinctive features and, hopefully some day, sememes. There have been challenging experimental contributions in psycholinguistics by brilliant scholars and experimentalists such as Miller *et al.* (1960), Brown and Lenneberg (1954), and Osgood *et al.* (1957).

Since Hebb's pioneering book on the *Organization of Behavior* (1949) in which a concept of structure in thought was given a hypothetical neural status, there have been numerous advances both on the neurological side, notably in the concepts of centrifugal control of information intake and on the side of behavioral changes predicted by the general Hebbian theory, in-

cluding the effect of early deprivation of experience and the studies of sensory isolation.

Still in its infancy but growing rapidly is the young field of computer programming theory whose influence is beginning to be felt through the elegant studies of Newell *et al.* (1958). What is particularly striking about these studies is the degree to which the step-by-step behavior of the thinker and the problem solver must be described and conceptualized before it can be simulated. The work has freed us from the rather bizarre compulsion to describe all behavior in terms of stimulus-response connections and, at that, to describe only those features of the behavior the psychologist thought well enough of to reward. Perhaps the new science of programming will help free us from our tendency to force nature to imitate the models we have constructed for her.

In addition, there has even appeared in these last several years a major work on personality treating man as a thinking and problem-solving organism whose troubles may reside in his ways of conceptualizing his world. I refer of course to George Kelly's stimulating *Psychology of Personal Constructs* (1955). A more detailed study of the relation of personality dynamics, and cognition has also come from the work of G. S. Klein and his collaborators (Gardner, *et al.*, 1959). This same period has produced a book in social psychology premised on the simple assumption that all of cognition is dedicated to the task of maintaining maximum internal coherence in our view of the world around us. Its author, Leon Festinger (1957), sees the greater part of cognitive life given over to the reduction of cognitive dissonances of various kinds. This theory is a bold venture and, while it scarcely does justice to the variety of maneuvers inherent in cognitive life, it at least attempts to deduce much of social judgment to a simple premise about the nature of cognitive functioning rather than leaving phenomena such as social valuation in an isolated and *ad hoc* status.

Finally two major works, one the product of the wisdom of a lifetime in psychology, the other a young men's book, have pointed to the planfulness of man's behavior, the necessity of taking into account what man anticipates and is set to do and the role of his thought processes in the establishing and executing of his plans. The first is Woodworth's *Dynamics of Behavior* (1958); the second, the courageous subjective behaviorism of Miller *et al.* (1960) appearing in their volume, in which the currents of work in human cognition, neurophysiology, computer theory, and linguistics merge into one still roily but vigorous stream.

All of these publications have appeared in the six years since the quadrennial review published by van de Geer in 1957. We are all too closely involved in these matters to have gained much perspective on them. On this occasion I shall look at three classical problems within the field of thinking in the expectation that they might have changed their shapes over the past several years as a result of the ferment.

The first of these problems I shall call the problem of organization. It involves a concept of cognitive structure and a notion about operations. When we work at a mathematics problem, for example, our performance seems to be based on a set of structures that we have firmly in mind such as number

series and the notion of an equation. What are these structures and how may they most fruitfully be conceived? As regards operations: the subject is presented with a set of givens, he reorders them into a structure, leaps beyond the given, arrives at an answer that he evaluates and returns with the comment "the problem is solved." What was he *doing*?

The second problem has, at least for me, some of the properties of a nuisance. Our elders and betters used to torture themselves with the question whether thought was "reproductive" or "productive": thinking that depended upon reproducing something we had learned before versus thinking that produced something new. The distinction was sometimes stated as doing something beautifully as compared to doing it stupidly, by insight, or by rote. In general terms: what is the role of memory in thinking?

The third problem: most of what we call thinking is something that can be predicted by knowing about human society and its products—language, logic, and myths—and nothing at all about individual human beings. In what specific way is this interesting?

The Problem of Organization

I begin by referring to a study by Piaget (1954) on the concept of object in his *Construction of Reality in the Child.* The general line of Piaget's argument, in my own words, is as follows. In order for the child to develop a concept of an object that, in the language of John Stuart Mill, contains the permanent possibility of sensation, the child must develop a concept of an object that exhibits at the very minimum the property of conservation of identity across displacement in space. This property of conservation in turn depends upon certain primitive properties of a general kind, such as separation of object from action, and of a special kind, such as the intuitive concept of continuity of a spatial field described in the Piaget-Inhelder book on space (1956). These underlying conceptual structures—and let it be clear at the outset that they are not morphological structures in any tissue sense, either wet or dry, but formal, nominalistic structures—are inferred from the behavior of a child looking for objects that have disappeared. To what extent and how does the child continue to search when an object is hidden and where? To what extent does the child act as if his own actions with respect to an object can cause the object to appear again? The Geneva style of experimentation, observation, and inference is well enough known to need little description beyond this. In describing the structures on which thought processes are assumed to be "based," and I put the word in quotations in honor of the nominalism of Piaget's enterprise, Piaget has recourse to the powerful language of logic, the propositional calculus. He says in his *Logic and Psychology* (1957, pages xvii–xviii): "The aim . . . is to study the application of logical techniques to the psychological facts themselves, and especially to the thought structures found at different levels of intellectual development. Theoretically, it is important to ask what kind of correspondence exists between the structure described by logic and the actual thought processes studied by psychology. The question of whether the structures and operations of logic correspond to anything in our actual thought, and whether the latter conforms to logical laws, is still an open one. . . . The algebra

of logic can help us to specify psychological structures, and put into calculus form those operations and structures central to our actual thought processes."

I am of a generation that was early warned by its teachers, whatever their nature, that the laws of logic are *not* the laws of thought, and my first reaction to Piaget's method was one of startled disbelief that so shrewd an observer could take so fatally erroneous a theoretical step. However, as Piaget has put it in his important little volume *Logic and Psychology* (1957), there are four ways in which one can go about thinking of the relation of logic to psychology or of the relation between the laws of logic and the laws of thinking. Two of them have minor psychological consequence and can be dismissed. The first is the Platonism of the early Alfred North Whitehead and Bertrand Russell, which postulates that logic is unrelated to experience altogether, is nonpsychological in origin, and relates to a realm of universals. The position requires that we ask how the child learns logic as much as that we ask how he learns the rules of a language. It reduces the problem to something less than an issue of correspondence and, while it converts it into a matter of learning, it loses general interest in the present context. The second position, also rather trivial, is the conventionalism that holds the laws of logic to be conventions. This position will be dismissed out of hand simply because it is altogether too miraculous that a set of conventions arrived at arbitrarily can have the degree of internal coherence that we know as logic. Again the issue is reduced to how we learn: this time learn the conventions. The third position is the logical empiricism of the Vienna group. It holds that one can distinguish analytic or syntactic propositions that are essentially tautologies on the one hand and, on the other, certain protocol sentences that have to do with direct experience. These are presumed to be empirical or synthetic truths. How one can separate them from logical operations remains obscure. There is too much observation indicating that an individual's experiences are not pure but constrained by the syntactic structure utilized: whether this is put into the form of the Whorfian hypothesis or into terms of some of Piaget's own experiments, which indicate that the child's experience of objects alters as a function of his implicit logic. In short, the Vienna group's position, although it may have been useful in cleaning up the linguistic transvestite confusion of syntactic tautology appearing in false empirical clothing, is of little use to the psychologist seeking a relationship between logic and thinking. At best, it would ask how one learns to impose syntactic structures on the givens of direct experience, which seems absurd when it is considered that experience is never had neat. The final choice is a kind of operational parallelism. A theory of mental operations is designed that studies the change in these operations through growth and development. An attempt is then made to examine parallels between the psychological laws of these operations and the logical operations of propositional calculus. In Piaget's case the mental operations themselves are described in the language of the propositional calculus: a point to be remembered.

I am firmly of the opinion that Piaget is doomed to success in this enterprise of finding parallels and correspondences, but his is a successful doom and an instructive one. For inasmuch as Piaget is using the language of

propositional calculus to describe the underlying structures of thinking, he cannot but describe them in that language. Having described thought structures and their supporting operations in these terms, how can he then establish parallels between the laws of logic and the laws of thought, between the structures of logic and the structures of thought? It is like saying that I shall describe the Harvard Yard in English and then determine whether what I have described corresponds to the modes of description permissible in English: it must, if I have so described it. At the very least I should need a metalanguage to discuss the suitability of English to my description. In the case of the propositional calculus, there is no metalanguage available with which to discover the correspondence between the rules of propositional calculus and the thought processes described in terms of it.

Why then do I say that the effort is doomed to success? Because I believe that what Piaget has done is to choose a language for describing the thought processes that is extraordinarily more powerful as an analytic tool than any natural language he might have chosen to use, either the three of his native land or even English. Indeed, given his firm conviction that one must consider the connected ensemble of the laws of thinking as a whole (his *structure d'ensemble*), then the propositional calculus gives him the language in which the connectedness of propositions is maximum and he is thus afforded a chance to put his ideas of the ensemble of operations to a test. Thus, if a child makes a statement of the form "pvq" the Geneva investigators are able to follow up with a search of whether the child grasps the full implications of that statement in terms of truth tables, other implications of disjunction, or similar terms. They can also test whether, when a child uses a certain kind of serial ordering of quantities that implies a grasp of transitivity, he actually grasps transitivity; they find that he always does grasp it and that it is not only logically but psychologically necessary that he do so. This finding is certainly not correspondence of logic and thought any more than that between motor behavior and geometry by virtue of the fact that for a person to walk from A to C, the two separated by region B, he must follow the route ABC. However, just as the study of operations in space is rendered deeper and more powerful by the existence of geometry as a method of analysis, so too is the study of thinking given more analytic force by the tool of the propositional calculus. In short, what Piaget has done is to introduce us to a very powerful means for describing and analyzing that which, in a highly connected way, a child does at different stages in development. In my opinion, this has been a truly major achievement and an advance over what has gone before.

Let me now contrast Piaget's style with that of another worker. Piaget's is principally a descriptive or structural method, a way of stating what the underlying structure of thinking must be if people are to behave as they do. The style to which I turn now is prescriptive: not only an attempt to say what has occurred, but to convert this knowledge into rules that might be used to mimic or simulate an occurence. I have in mind the work of the Carnegie-Rand group, notably Newell *et al.* (1958).

These investigators introduce their searching paper on problem solving and its simulation with the following remark: "What questions should a theory

of problem solving answer? First it should predict the performance of a problem solver handling specified tasks. It should explain how problem solving takes place: what processes are used and what mechanisms perform these processes. It should predict the incidental phenomena that accompany problem solving, and the relation of these to the problem solving process. For example, it should account for 'set' and for the apparent discontinuities that are sometimes called 'insight.' It should show how changes in the attendant conditions—both changes 'inside' the problem solver and changes in the task confronting him—alter problem solving behavior. It should explain how specific and general problem-solving skills are learned, and what it is that the problem solver 'has' when he has learned them." For the Carnegie-Rand group, the most proper way to look at the processes of problem solving is in terms of information processing systems described as programs for treating information. These programs consist basically of a set of symbolized memories interconnected by certain clearly specifiable ordering relations, a number of clear and definite primitive information processes that operate on the memories (such as "enter," "match," "erase," "exit," and compounds of these) and, finally and most important, a perfectly definite set of rules for combining these primitive information processes into programs of processing. These programs *are* the basic theory of thinking or problem solving: they are not analogies. In the words of these authors (page 153): "We wish to emphasize that we are not using the computer as a crude analogy to human behavior—we are not comparing computer structures with brains, nor electrical relays with synapses. Our position is that the appropriate way to describe a piece of problem-solving behavior is in terms of a program: a specification of what the organism will do under varying environmental circumstances in terms of certain elementary information processes it is capable of performing. This assertion has nothing to do—directly—with computers. Such programs could be written (now that we have discovered how to do it), if computers had never existed. A program is no more, and no less, an analogy to the behavior of an organism than is a differential equation to the behavior of the electrical circuit it describes." The Carnegie-Rand group has developed and refined an information-processing language whereby the primitive processes can be combined into programs and, indeed, they have developed a highly effective Logic Theorist quite capable of proving theorems in *Principia* language, and in fact capable of discovering theorems. Their theorist makes errors, as do human beings, and in no sense is it a genius in the science-fiction version of a computer program. It is an interesting simulation. The program includes some programs or routines of operation—such logical operations as substitution, replacement, detachment, and syllogism—and these are arranged as a kind of list-structure specifying, for example, which things should be tried first.

How such programs are built is interesting. The general premise is drawn from Turing's theorem that anything computable, in the sense that an answer can be obtained in a finite number of steps, can be computed by a machine with a very limited number of primitive processes such as entering binary digits, erasing them, and matching. However, simulators are not interested in building efficient machines that can compute anything computable. They

watch behavior (such as thinking) very carefully—including introspection on their own behavior—and then try to describe the steps they have taken in the process. They then try to use a Turing machine to reconstruct those steps, and the art of talking to the machine in terms of a programming language depends in considerable measure upon how well one has stated what the behavioral process has been. This is how the Logic Theorist was built, as were various chess-playing simulators and similar machines. What simulation consists of is not simply describing something in general, or even describing its underlying structure. Rather, it consists of writing an efficient sequential blueprint in a specialized language that reproduces a play-by-play account.

Here then is a second device for analyzing structure: but structure now described in terms of a set of operations to be performed. The operations can be as various as one pleases. Indeed, there is no reason why a program could not be written for the 16 binary operations of the propositional calculus on which Piaget so heavily depends in describing the formal operations of the emerging adolescent. There is every reason why this should be done, for there is one special advantage to be gained by attempting to translate Geneva into Carnegie-Rand. In doing so, it would be necessary to specify how the various structures are used, in what order, and how the ensembles of rules are ordered in terms of priorities for use. It would have the effect of rendering the Geneva approach not only less Kantian but much more closely related to the sequence of acts involved in thinking, problem solving, and cognition generally. Elsewhere (1959), I have suggested that Piaget leans too heavily on the concept of equilibrium (or, rather, disequilibrium) to get his organism into action. I noted at that time that the system he had constructed so brilliantly had too little place in it for the description of the strategies and tactics of thinking that it described the structural constraints upon thinking but not the plans that guided thinking.

My first suggestion, then, is that some effort be made to join these two powerful trends in the analysis of thought processes: the descriptive approach of Geneva with its exquisite and penetrating use of logical analysis of behavior, and the prescriptive approach of Carnegie-Rand with its insistence by definition on the actual processes that unfold as a person of a given age faces a concrete task.

Shifting the discussion of structure to the side of neurophysiology, I think that the easiest way to put the matter is to say that the neurophysiological problem of structure is solved with respect to the thought process at the moment one is able to describe a nervous system as having the properties that allow it to be a computer and fulfill the properties necessary for Turing's theorem. The nervous system then places no further constraints on behavior, and all that is needed is for the psychologist to tell exactly what kind of behavior he wishes to describe, construct a program or set of programs that fulfill the requirements, and then set a job for the computer simulator and the neurophysiologist alike. In a system that has, as has the human cortex, some 5×10^9 elements each in effective contact with 20 elements at each end, it should be possible to describe programs for the most complicated

of problems: as long as they are well defined. As George Miller and his associates put it (1960), there is no reason to believe that God issued us an insurance policy against complexity. The nervous system, we may properly conjecture, is not going to be much simpler than the behavior it underlies. McCulloch and Pitts (1943) have at least provided an existence theorem to prove that a nerve network given its known properties could fulfil the properties of a Turing machine. However there is a long way between an existence theorem and the thing in existence. All we can say is that Hebb's work (1949) and the work that it has stimulated may provide the beginnings in such doctrines as the cell assembly and phase sequence.

What I am trying to say is that I do not think that we should look to the neurophysiology of thought to solve our problems for us. We shall not be able to tell the neurophysiologist what to look for until we have a much better conception of what it is that goes on in thinking. We should do well for the time being to be as rigorously descriptive and prescriptive as possible. Then we can call in our neurophysiologist and ask him how a system as forbiddingly flexible as the nervous system is actually might produce the results found.

Let me give a few examples of the conversion of description into prescription. Let me mention several pieces of work going on in my own laboratory in order to illustrate the difficulties. The first problem has to do with the cost of hypotheses balanced against the gain obtaining by using them. In the first experiment, we use a very simple situation, one involving the simplest of cognitive reactions: identification or categorization of an input. In the main, distinction can be made between hypotheses about the identity of an input that are based on a considerable amount of prior information, in contrast with those based on minimal information. There is some optimum point where an hypothesis is at its maximum utility. If a hypothesis is offered too early and on the basis of too few cues, the investigator must test it and, in doing so, he may possibly overlook new and important information not relevant to the test of the hypothesis. For several reasons, there is also a point at which it would be inefficient to hold back the offering of hypothesis for testing. Information may be getting obsolete, the absence of an organizing hypothesis may lead to informational overload, or the hypothesis might be correct, in which case the worker is only paying a price for delaying. In this experiment we present one group of subjects, one at a time, with pictures that are slowly moving from out of focus into focus over a period of four minutes per picture. The pictures are perfectly familiar scenes, such as fire hydrants, common tools, or tableware. The group of subjects is told to report hypotheses whenever they have them about what might be in the picture, regardless of blur. One of the first things noted is that some subjects suffer the phenomenon of functional blindness and, even after the picture is in full focus, will sometimes sit for as long as two minutes without recognizing it. My research assistant, Philip Daniels, has put me through the procedure with pictures I had not seen before and I have experienced the confusing state. I have some introspective observations. It is the hypothesis formed on what appears to be "just short of the right amount of informa-

tion" that stops further thinking and that occasions hard testing of the visual display. Hypotheses formed on the basis of the early blur have little permanence. We shall know shortly whether this is so.

Various workers—notably Selfridge and Neisser (1960)—are now attempting to construct programs that will in effect simulate how humans recognize objects or displays. The experiment I have described suggests that if simulation is the object a set of rules should be written into the program that specifies the consequences of making decisions about hypotheses at different times. Immediately it would become necessary for the over-all or metaprogram to specify the objectives of an identifier: whether the purpose is to identify, quickly, with the attendant risk of inhibition, or to win little but fail safely, so to speak. This program would determine how much cue input was required before the program permitted the formation of an hypothesis. It cannot be too strongly emphasized that, in the writing of a prescription for an identifier, this feature of behavior cannot be overlooked. Such an oversight would be like a recipe for baking a pie that did not specify when to take the pie out of the oven. Unfortunately, there has been no such compunction among descriptive analysts or theorists. *Gestalt* theory, which I do not wish to use as a whipping boy, for it has contributed greatly to our grasp of cognitive phenomena, has long been indifferent with regard to the set of the subject. If it had had to prescribe for a perception simulator, would it have been able to do so? I submit that description can be partial more readily than prescription can, for the results of partial description are not as apparent to the naked hypothetical eye as the wildly varying results of improper and partial prescription.

To mention a second feature of the timing of hypotheses: we have used a procedure that is little more than the old game "Twenty Questions." The procedure is carried out with 12-year-olds, and their performance is carefully studied as an example of information processing. In the opening play, so to speak, there is an option at the start of a sequence between trying out hypotheses and establishing constraints: where the problem has to do with a car going off a road and smashing into a tree, the difference between the question, "Was the driver blinded by the lights of an oncoming car and went off the road?" and "Was it at night?" Again, there is a significant correlation between the number of constraint-locating questions asked before the first hypothesis and the likelihood of solving problems. This initial balance of constraint location and hypothesis testing is also related to the extent to which questions throughout the problem utilize, violate, or ignore information already obtained from prior questions. In short, a kind of parameter can be specified for any given individual concerning the extent to which he is cumulative and constraint-sensitive in his information gathering, in contrast to noncumulative and risk-taking in the use of hypothesis. Again, rather than simply describing these processes, we must ask ourselves how we may program this characteristic of the problem solver, that is, program it almost in the sense of the style variable that it is. What is suggested is another list structure with style given by the order of tryout of different approaches to a problem.

I have said enough now about the problem of organization to make clear

that I think that we are at the dawn of a new day in which the character of psychological theories of thinking will change drastically to the form of using more powerful descriptive tools drawn from logic and linguistics and, using them in such a way as to end up not simply with a description of behavior but also with a prescription or program for reproducing it.

Memory and Thinking

A brief paper of this kind permits only a cursory look at so complex and classic a problem as the relation between memory and thinking. Let me sketch briefly what seems to me to be the new shape of the problem in the light of the work of the past decade. The point of view toward memory that I wish to take is based in considerable measure on an approach to the subject developed by my colleague George Miller, and in what follows I shall probably fuse his ideas with my own since we have long had this mutual interest. Let us begin by assuming that, if we stored in memory all we had experienced on some kind of psychic tape recorder, it would do us little good; for to recover it, we should have to play the whole thing back, which might take as long as life. Let us assume further that, if it all entered our memories by random means, we should still be without a ready method of recovering it. We should not know where to look for it. Let me turn now from this statement to the assertion that the interesting thing about the storing of information in the memory is the manner in which it affects our capacity to retrieve the information or, as the case may be, to reconstruct it in much the same sense as one reconstructs information about falling bodies from retaining the formula: $s = \frac{1}{2}gt^2$. Under these circumstances, the brute memory problem is trivial. The real problem is organization, for it is organization that makes information available when needed. All the other effects—interferences forward, backward, and within—are comparatively minor.

What seems to me to be most characteristic of the behavior we call remembering is that it consists of relating newly encountered inputs to cognitive or semantic structures that have already been formed or, when none exists, designing a structure that will take the new information. We have been studying the paired-associates learning of young teenagers, giving them pairs of meaningful words with the instruction to remember one when the other is presented, and not to confuse the pairs. These youngsters' success in being able to recover the material seems to depend upon whether they can find a good structure for linking, a process familiar to them. They even have preferences for structures. When we ask them to write down what they are doing, they easily jot down the mediators they use. We can order our subjects on at least three Guttman scales in terms of their preference for thematic mediators, equivalence mediators, and part-whole mediators. Thus for *chair-forest*: "the child is sitting on a chair in the middle of the forest," "chairs and forests are both made of wood," and "chairs are made out of forests." When material is recoded or structured in a person's preferred mode, it is more easily recovered on request. However when it is put in *any* kind of structure, it is more easily recovered than when it is left unstructured.

Let me now pose a curious question: Why are mnemonic devices of limited use? Such devices are helpful, to be sure, when the task is to recall material

on request, as in our typical memory experiment. Nevertheless they do not guarantee that material will be recalled in connection with a task in which they are needed. It would seem that, in order for information to be recovered in connection with the fulfillment of a particular task, it is required that the information be organized in some context related to that task: not isolated from it. In short, the best mnemonic device for remembering something in the context of a particular task is to store it in the same context or structure as that task, or, at least, to place it in a territory close to that required for the execution of the task. One of the ways of assuring that material will not be available for use is to fix it functionally in a context that captures or masks it: as in the experiments of Maier (1930).

One further point before we face the problem of productive and reproductive thinking. Material can be retained in a way that makes it either manipulable or inert with respect to the manner in which we can perform transformations upon it, as in combining it with other material, performing inversions, or obtaining its contrast. This is again a function of how it is structured at the time one gets the information. In the Twenty Questions game described earlier, some subjects carry information along with them by the use of economically wrought summaries that are altered as they go in an orderly fashion: for example by successively crosscutting the domain of possibilities. Others create so much internal confusion by loading on needless information that they soon find themselves unable to integrate what they have, the chief tactical villain here being the need to obtain information about why an hypothesis was wrong. This is also the case in the behavior of people attempting to attain concepts in the experiments reported in *A Study of Thinking* (1956): some used strategies such as simultaneous scanning that in essence got information organized in a way that rendered it almost unmanageable and inert, whereas others put it into a structure that could be altered systematically and without strain as in conservative focussing. A "good" memory organization, I suppose, is one in which information can be transformed for use on a broad range of particular tasks of the same or related type.

When we speak of reproductive thinking we are not necessarily speaking of a rote performance in which material stored is regurgitated in the task assigned the subject. It is altogether an achievement that relevant material is brought up and used appropriately. Despite all our studies of memory in the context of recall and recognition task, we still do not have a very clear sense of what increases the likelihood that relevant material will be recovered for use in solving particular problems or in carrying through a given line of thought. There are some exceptions to this statement, notably some striking experiments by I. Müller reported by Köhler in his *Dynamics in Psychology* (1940), and also the experiments of von Restorff (1933). Müller at least shows a condition that alters the likelihood that a given mathematical rule will be applied to the solution of an algebraic problem: what makes for imbedding in a cognitive structure and what makes for isolation. It would be much more to the point to re-examine the literature on transfer of training, particularly transfer of principles and sets, to determine what leads to carry-over of information to new tasks and what does not. There are far too few studies such as those of Maier, Birch and Rabinowitz, and Müller that shed light on the conditions that make

a particular piece of knowledge accessible to a particular task and not to another. In so far as the psychological literature is concerned, "merely" reproductive thinking is as much of a miracle as so-called productive thinking.

As regards productive thinking, I think the issue is not one of inventing something new, but rather of structuring information in such a way that it can be transformed, permitting the person to go beyond the information given to some new consequence by induction or deduction. I think of this type of thinking as obtaining the full measure of redundancy from the information we have at hand. For such a gain to occur, the remembered material must be in a form susceptible to operation, it must have a manipulable structure. As we say to our students: "It is not just what you remember, but what you can do with it that counts."

In effect, then, the old problem of productive versus reproductive thinking is really a problem of how information is organized for storage and how that organization affects retrieval and utilization under a variety of conditions. What is sorely needed is a new look at the conditions of organization as they affect both the accessibility of information and its transformability for new uses. I cannot believe that the paradigm of proactive and retroactive inhibition has very much relevance to these matters or to the development of habit strength of specific responses. Factors of this order are more and more striking as the material to be remembered is less organized and the more the task is "pure recall." Dealing with highly structured bodies of knowledge, the issue is much more likely to be as we have indicated above.

I even suggest that we are too much under the thrall of our older theories of memory: much like the subjects that become inhibited in the experiment on identification of figures coming into focus. Perhaps in the interest of refreshing our approach we might try to *design* a memory system, for example, to write some prescriptions for the organization of a library of a specialized kind. How would information about physical chemistry be stored for example, in order to make it not only more accessible for specific uses but also to free the material in a way that would allow for maximum combination with other information? In what kinds of units would it be stored? Would the most frequently used material be put at the most accessible location or would it be reproduced and put at a great many locations? What should the rules of cross-referencing be, and in what order on a list should cross references be placed? Should there be a metalibrary of the major comprehensive ideas for quick scanning to guide us to more detailed information that we might need? How many such ideas should be included and organized according to what principles? Might such an exercise provide some light on the false leads that lure us in mistaken directions of pursuit?

Language, Myth, and Thought

Words, as Roger Brown has recently reminded us (1958), are invitations to form concepts. Sentences, it might be said, are invitations to combine concepts in a particular way. Natural grammars and the structure of a language provide the temptation if not the necessity to isolate, contrast, and organize general experience in a certain way. Myths, models, or theories—I use the terms interchangeably in this context—provide perhaps the most general and

blanket invitation for experiencing and operating cognitively in a certain way. If it is true that life imitates art, it is true by the same token that nature imitates science. All of this is very familiar ground, and I refer you to such familiar studies as those of Brown and Lenneberg (1954), Lenneberg (1953), Werner and Kaplan (1950), Osgood (1957), and others. Recently Luria (1959) has contributed two important papers to this growing literature in which he adds another important dimension: the pragmatic function of words. He points out that language has the effect, once the control of behavior is transferred to language systems, of freeing us from the control of stimuli. Not only do events become related by principles other than the simple generalizations of conditioning—freed from the first signalling system of I. Pavlov—but the system of relating inherent in the syntactic and semantic structures of language comes under the control of the subject in the sense of providing a self-stimulus that can be used to free him from the dominance of reactions established to linguistically uncoded environmental stimuli. It is at this stage that Luria remarks that behavior passes to the control of the so-called second signalling system.

It is in this sense that I remarked earlier that a knowledge of the language and the myths of a society could give an excellent basis of predicting individual behavior without knowledge of specific individuals. It is by now a banal point, but it has some important consequences for the study of thinking and for the specific experiments we carry out.

I mention only one of these, leaving the general problem in abeyance: the problem of discontinuity and contrast in language as these affect the thought processes. Characteristic of all sorts of phenomena in language is the idea of discontinuity. The phoneme is a case in point. In hearing a phoneme we accept a wide equivalence range of variation in spoken sound, except in certain specific distinctive features of this speech flow (Jakobson *et al.*, 1952). When there is that variation in distinctive features, the word changes meaning: for example, when we go from "pat" to "pet" with all sorts of variations in the voiced or unvoiced quality of the "p" sound and the "t" sound acceptable. The ranges of places in the mouth or throat, or on the lips for voicing a particular phoneme are also discontinuous. There appears to be a similar discontinuity in morphemes and lexemes, although here the case is not as clear. Similarly, in language it is found that contrast prevails as an important property. The distinctive features we listen for in distinguishing phonemes are binary contrasts such as voiced-unvoiced, aspirated-unaspirated, and acute-grave. In words proper the meaning changes when the implied contrast changes: man versus standing bear, man versus ape, man versus angel, and man versus everything else. The structure of most languages, then, seems to have built into it a discontinuous set of distinctions that are organized in terms of contrasts or opposites.

George Kelly (1955) has suggested that human concepts used in organizing our personal world are similarly of a contrast type, and he has proposed the method of triads as a technique for getting at these contrast concepts: each with its positive and its contrast pole. The method consists of taking three terms, asking which two are most alike and in what respect, and then asking in what respect the third differs from the two similars. It is a method that has had considerable success in linguistics. Kelly uses the technique to find out

how his patients structure their world and among his most valuable data are the so-called latent contrast poles employed: the unrecognized contrast classes employed by his subjects.

Kurt Lewin (1946) used to urge that one of the important aspects of personality was what he called cognitive structure. I believe that we now have a technique for preliminary mapping of cognitive structure. It is precisely the method of triads with the techniques available for reducing the domain of responses to a set of general contrast dimensions. I suggest that it would be of the greatest interest to attempt to map a cognitive area in which a person was later required to solve a problem or think through a train of reasoning to see the manner in which the discontinuous contrasts operate. We have done some preliminary, quite informal, work of this kind in which subjects classified a group of famous men in terms of the method of triads. We have then asked the subjects to discuss the ways in which they influenced each other and the world. It is interesting to see the manner in which their accounts reflect the structure of their concepts. It would be easy to do the same thing with respect to a set of tools or implements to be used in a Maier-type problem. Can isolation and functional fixedness be predicted?

I have mentioned this particular problem with a single thought in mind. For too long we have paid lip service to the effect of language and social myth on thought, or simply touched upon the matter by studying the manner in which social convention affects syllogistic reasoning. I think the time is ripe to examine in far fuller detail the manner in which the structuring principles of language, science, and myth—not the content but the structure—affects our manner of operation in thinking. Again, as in the case of the propositional calculus, there are powerful tools available for getting at these structural principles. There is much information available and there are already investigations—if I were to choose one it would be the Brown and Lenneberg study on color nomenclature (1954)—that point the way.

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Discussion of the Paper

Question: You mentioned the Turing theorem or machine; if that is in my memory warehouse, it certainly cannot come out now. Will you bring it out for me?

Bruner: Yes. I cannot quote it exactly but it states the following: whatever is computable, which means anything for which an algorithm of search may be set up so that an answer in an ensemble of possible answers may be found can be computed by a machine with a minimal number of operations.

You can build a good computer program, by the way, by just getting a bunch of cardboard chips that have zeros and pluses on them and a series of racks in which to place them. You write specifications about how to operate and enter a 0 to 1, erase it, move it when you encounter a certain sign or sequence, and make other changes.

Simulation does not use very much of the apparatus of the Turing theorem. It simply tells the worker to observe behavior and then build up a subroutine that looks like it. This subroutine can then be used as a unit. Let us say this routine is adding; now a superordinate system is constructed with subroutines such as adding, inspecting, comparing, and obliterating. Depending upon your theory of repression, a model for repressing can be built. However it must be made explicit if it is to be built.

Jean Piaget (*University of Geneva, Geneva, Switzerland*): I agree with Bruner's point about the relation of axioms in logic and the nature of thoughts. What we seek to do is to emphasize parallels in the algebra of logic and the operation of thoughts. What he says about the use of programming theory, I agree with completely and wish to urge that we make an attempt to use it.

BRUNER: There is a possibility that the neutrality of computer theory can be used, namely the fact that you are describing programs as a kind of meta-language in which you can show analogues and correspondence even between logical operations, cognitive operations, linguistic operations, and the rest that are performed. The only thing that can be said is that it is going to take at least five years to find out whether what I have said this morning is nonsense.

THINKING AND SPEAKING

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There has been since the time of the ancients a never-ending discussion about the relation between speaking and thinking, particularly concerning the question of whether speech and thought represent an indivisible unit or whether we are dealing with two fundamentally different mental activities. Both viewpoints are to a certain degree correct in so far as they explain some phenomena, but consideration of the whole field of experience from the point of view of a monistic or dualistic theory was not satisfactory. I do not consider it my task to contribute something to the solution of this intricate problem by a theoretical consideration of the pros and cons in relation to the different opinions. I think it may be more useful to report my inquiries into the way language and thinking appear in a concrete situation, particularly because in this respect I can refer to personal experiences of long standing that gave occasion to, even demanded urgently, study of the interdependence of both phenomena.

The situation that brought the problem to the center of my interest was a very practical one. During World War I and for many years thereafter, I was confronted with the task of retraining a great number of persons with severe defects in thinking and speaking due to a damage of the brain cortex.[2] In some of them the defect of thinking, in others the defect of speech, was predominant and my associates and I had to decide in which realm we should begin retraining. Therefore it became essential to formulate a definitive attitude toward the problem of their interrelationship. More correctly, we learned from experience—particularly from the failures in our undertaking—much about the problem, and I think we advanced in our theoretical concepts.

That my studies were based primarily on the observation of pathological phenomena was met with skepticism. I have tried to show on different occasions[4] that my procedure not only permitted but was even highly conducive to understanding normal behavior, at least when it was performed with evaluation of the modifications that pathology brings about.

Referring in this respect to previous publications I want to mention here only that I came to the conclusion that what appears to be pathological can be considered normal behavior modified according to definite laws, and that it is determined by the same basic motivation as normal organismic behavior in the trend to exist, that is, to realize its possibilities and its nature as well as possible under the given conditions and under the condition rendering the organism pathological.

Our treatment of the patients was, in the beginning, very unsatisfactory as long as we considered—according to the theory of the time—the defects of the patient as isolated phenomena and based our retraining on them alone. It improved considerably when we tried to apply to the study and treatment of these patients the so-called organismic approach to biology[3] that I had developed at that time, developed to some extent from experiences with patients with speech defects. That meant methodological examination of each

patient as much as possible in all performance fields (not only that appearing defective), so to say, the whole personality. In respect to our problem the investigation of the nonlanguage behavior, also more or less disturbed in our patients, became particularly important. Such a procedure not only produced much better results from our training but gave us much insight as well into the nature of language and the relationship between thinking and speaking. From this general point of view we came to the conclusion that each thinking and speaking performance has to be considered as a means of the individual to come to terms with the world in his attempt to realize himself as much as possible. From this point of view the problem of thinking and speaking revealed itself in a new aspect: we realized it could no longer be our endeavor to consider thinking and speaking as isolated phenomena and bring them into relation to each other, but to consider both as different ways of fulfilling the same goal of the individual, the concrete task with which the individual is confronted when sometimes thinking, sometimes language, has to be put in the foreground.

I start with the analysis of a seemingly very simple disturbance of speech, namely the frequently observed inability of patients to find the name for objects, even for the most common objects of everyday life. This clinical picture is usually termed amnesic aphasia.[4] It is easy to demonstrate that this defect is not based on a disturbance of recognition. The characteristic circumlocutions that patients offer when they are not able to name an object leave no doubt about it. For example, a patient who could not name an umbrella, a glass, or a penholder replied, "a thing for the rain, a thing for drinking, something to write with." It became evident further by the fact that the patients selected objects from a group without any difficulty when they were called for by name.

It has been the assumption—according to the theory that language is based on the awakening of images of words deposited in the brain—that the disturbance in finding words for objects is conditioned by a defect of such images or is caused by the fact that patients have lost their ability to evoke them. However, such an assumption is not in accord with other findings that prove without doubt that the patients have not lost the words. They are able to use them in conversation or in a description of a situation. For example, a patient who cannot name one color among several, let us say the color green, uses the word green without difficulty in reciting the different color words: red, green, yellow, and blue; or the patient who could not name the umbrella says, "I have three umbrellas at home," but even so is unable to name the umbrella shown to her immediately thereafter.

If we say that the patients have not lost the words, what then makes them unable to name the objects? The answer to this question came from observations of the total behavior of the patients. They are attuned in an extraordinary way to their own person; they are, on the whole, acting more than thinking and speaking. Their speech is accompanied to a high degree by expressive gestures that often appear to take the place of missing words. The character of the change in their behavior became especially clear in the examination by certain tests[6] that my co-worker, the late A. Gelb, and I had developed for the purpose of studying the general attitude with which an individual faces

the world. Some object-sorting tests proved to be particularly useful for this purpose.[4] One example may illustrate the patient's behavior: confronted with a great number of woolen skeins of different colors, different shades of red, green, and other colors, the patient was asked to select all shades of one color skeins to find those that matched and that seemed similar. A normal individual under this condition rapidly selects a great number of different shades of the basic color presented, disregarding differences of shades or other striking qualities of the color skeins. The patient, however, could apparently choose only completely identical or very similar ones belonging together according to their brightness, coldness or warmth, beauty, or usefulness in a definite reality situation. Thus, for example, a woman chooses a green and a red to a blue one and, when asked why, explained that the green is for the skirt, the red for a blouse going with it, and the blue for a shawl. Sometimes a patient, attracted by the brightness, put together a light green, a light red, and a light yellow. It might have been thought that he were color blind if further examination had not shown that he could distinguish all colors very well.

The defect of the patient apparently consisted of an inability to sort out, according to a definite category, the basic color quality. Even if his activity seemed to be determined by something that may have appeared to us a special attribute—for example, brightness—that does not mean that he selected with respect to the latter. This was evident when he was unable to choose all bright skeins having specifically been asked to do so.

The patient's behavior can be understood when it is known how we ourselves fulfill the task. When we choose, for example, all various nuances of red, even those that appear very different, we do this because they belong together with respect to the basic color quality. The several shades appear to us not as individual but representative of this quality. We ignore all experiences except the specific character represented. We disregard them even if we become aware of them. We say we are able to do that because we can abstract[7] and so hold fast the direction of procedure necessary for fulfillment of the task.

Normals are able to behave differently to the world and thus also toward the heap of colors in the test. We can, so to say, give ourselves over to the sense impressions passively; then we experience those color skeins that are equal, that cohere, and immediately stand out from the heap. If not many equal ones are there, those that are similar in one or another aspect come passively to the fore and we feel some unrest in the heap, while in choosing according to a definite category, those that belong together come definitely to the fore and we scarcely pay attention to the other ones as they are in the background.

From the results with these tests and the observation that patients under everyday conditions also behave principally in a similar way as in the tests—that is, they are able to react normally only to any condition that does not demand abstraction—we came to the conclusion that we have to distinguish two attitudes in man's orientation to the world, the abstract and the concrete one.[7] To clarify the difference further I may state that in the concrete attitude we are directed toward the actual thing in its particular uniqueness, while in the abstract one we are determined by a principle, a category, or a concept, in which the objects are only incidental examples and representatives. The

patient's behavior is similar to our concrete attitude. We say, therefore, that they are impaired in their abstract attitude and have become beings dominated to an abnormal degree by concrete promptings. We felt further justified to assume that the inability to name is an expression of the impairment of abstraction.[4] This assumption of the structure of naming is confirmed by our experiences. When we name an object, let us say a table, we do not mean this particular table with all its properties but table in general, the concept table; the word is used as a representative of a category.

This assumption is further confirmed by the fact that in concrete behavior language does not play the same role as in abstract behavior. In concrete behavior language does not occur at all or only accompanies our activities; the word is not much more than another property of the object, such as size or color. In a situation where we have to act abstractly, words are very important as they induce us to take an abstract attitude; the approach to the world in a conceptual way is close to language.

Our conclusion about the nature of naming became very relevant for the theory of language. Words are apparently not tools that may be handled like objects, but are means to help man to organize the world in a particular, conceptual, and symbolic way.

Although stressing this character of language, we should not forget that it is not the whole of language. Words can also have another character. This fact was again revealed by observation of our patients when they used correct words in the task of naming an object in spite of their having a defect of the abstract attitude and in spite of the fact that analysis showed that their words had not the character of meaning and did not represent concepts, but were much more closely adapted to the individuality of the object. The essential difference of the words they used in naming became apparent when the patient did not utter the generic words such as the word red but used such words as rose-red, grass-green, strawberry-pink, or sky-blue. We call these words individual words because they fit only definite objects. They represent simple associations of sound complexes to a particular object. The patients will use more such words if they have them at their disposal from their premorbid life.

It could be shown that even if, in answer to the object-naming task, the patient utters a word that at face value would appear to be a generic word, it does not have this character for the patient. Thus, for example, if a patient seems to be able to name a chair, he can utter this word in relation only to a special chair; he is not able to call other chairs by the same word, which we can do without difficulty. Apparently the word does not fit all chairs, does not represent a category, and is not a name.

The situation becomes more complicated when we observe that the patient even after training may call the different shades of color in the test with words identifying the basic color quality, for example, all shades of red would be called simply red. It might be thought that the patient had regained the abstract attitude, but examination showed that this was not the case. The question arose as to whether he could have acquired the symbolic character of the word in spite of the defect in abstraction; that this concept was wrong may be illustrated by the behavior of one such patient. When we asked her why she now called so many different skeins red that she never did before and

whether the word red really fitted all these different shades, she laughed and explained emphatically, "No, not one is red, but the doctors have told me that all these skeins are red, therefore I use this word to please you." It became evident that she had not regained the symbolic character of the words but had only, by her excellent memory, acquired a superficial association between one word and different objects. In the test of naming we call this use of words pseudonaming.

In some patients we find a defect of these associations, that is, of the instrumentalities of language, for example, in those forms of aphasia that usually are called motor or sensory aphasia. Here an inability to use such associations can simulate a naming defect. The difference from the other kind of patients is that these latter understand very well what naming means. This difference shows, for instance, if, at a request to write down the name of an object, they are able to do so without difficulty, while a patient with a defect in the function of naming is not at all able to accomplish this task.

The patients with a defect in naming show characteristic modifications of their language in other respects. They have difficulty in starting to speak, in shifting from one context to another, their vocabulary is reduced in general, their language has changed from an active, spontaneous means for expressing ideas and feelings to a passive, more or less compulsive stereotyped reaction form to definite stimuli: in short, the effects of the impairment of abstraction. I cannot deal with this in detail here and it is not necessary to do so in connection with our problem of thinking and speaking. In all these respects the patients with difficulties in finding words in motor aphasia behave characteristically different.

We can distinguish, on the basis of our experience with patients, two different forms of language: the language proper in which the words have the character of symbols; and the language consisting of learned motor and sensory performances that we call instrumentalities of language. In living speech we use symbolic language and instrumentalities intermingled. Now, it is very important to realize that the words uttered in one or the other form can, at face value, seem very much the same. Thus, a correct interpretation of what the speaker wants to communicate will be possible only if we consider his speaking in connection with the total situation in which it occurs. Only then will we be able to make a decision as to whether we have to deal with the presentation of learned material or if the language represents thinking.

Turning now to the discussion of the interrelationship between thinking and speaking I stress first that thinking is not the only factor besides language that determines man's behavior. The influence of a number of other factors is usually spoken of in psychology, including images, interest, emotions, attention, memory, and automatisms. I have from the organismic approach criticized the assumption of such isolated capacities and tried to show that all of these apparent determining factors can be understood from the viewpoint that the influence of that that is best suited to guarantee self-realization comes to the fore: in pathology in spite of the defect and in normal life in abnormal difficulties. That can be exemplified, for example, by the significance of the "images" for performance. Previously abnormal behavior has mostly been interpreted—as have been aphasic symptoms—as a result of a damage of images of words.

This interpretation has now nearly generally been abandoned as far as the abnormalities of "language proper" are concerned. It was found that the apparent inability of the patient to elicit images is not the effect of the damage of the image mechanism by pathology but of the impairment of the abstract attitude. That the images are not lost is shown by the observation that patients who are not able to elicit them on demand use the images passively in their performances without being aware of it. In cases where the abstract attitude is intact the patient may compensate a defect of the instrumentalities even by voluntary use of images.

The same difference concerning the active and passive appearance of these "influences" are observed in memory, emotions,[5] and other mentioned conditions where one of these assumed factors appears to be defective.

Concerning memory, patients with defect of abstraction are mostly very poor in rote learning in all fields. Even if they have gained some results—which takes place with unusually great effort—they usually forget what they have learned after a short time when it is not used continuously through adequate stimulation.

Both incidents reveal the relationship of the acquired automatisms to the abstract attitude. It could be asked how patients with impairment of abstraction, who are capable only of the concrete form of behavior, can do anything at all if their activities are not instigated by outside stimulation. As a matter of fact, the patients are very restricted in their activities. They have to be brought into action by the people around them. Their performances are correct only if the situation is arranged in a way that the patients can react in concrete behavior forms. Thus it is very important that, if the performance capabilities of the patients in respect to thinking and speaking are to be known, attention be given to this condition. It could be said that their activity depends on the abstract attitude of the normal individuals, of the "others."

We are confronted here with a problem that concerns not only the patients but all individuals who are restricted to the concrete form of behavior. That is the case, for example, with children, individuals in anxiety states, and people in primitive societies who are considered to be living on an inferior mental level.[8] The abstract attitude of the "others" plays an essential role in the behavior of all people with impaired abstract attitude. This conclusion, described in more detail on another occasion,[11] points to a definite concept of human nature, namely, that to man's existence belongs the state of living in communion with others. We have to consider that when we want to understand thinking and speaking and their interrelationship. The social character of language so often stressed is an expression of this fundamental core of man's nature.

In the foregoing discussion I have shown that thinking is not the only, if however the most important of, the nonlanguage "factors," and that these factors, like thinking, represent different forms of the function of the unitary process of human behavior.

Thinking is such a complex process that I cannot try to characterize it in all respects, even superficially.[4] I have to restrict myself to some general remarks, discussing particularly those phenomena that are important for the interrelationship between thinking and speaking.

Thinking is instigated by perceptions, concepts, feelings, thoughts, and ideas. It goes on in the form of analysis, selection, and synthesis until a uniform result is achieved by means of which the tasks with which the individual is confronted can be fulfilled. It appears in a form that can be called a sentence. Thereby, not only present events play a role, but also the remnants of previous thinking that were fixed as thoughts in the form of automatisms. They become more or less passively effective, facilitating the active process of thinking. Thus we have also in thinking to distinguish an active and a passive part. The fixated thoughts often appear in words. That form occurs particularly when the concern is not so much with creation of new concepts but with reproduction of old ones for special purposes, for example, in communication of our thoughts to others. In accordance with what we have discussed before, it is understandable that it sometimes may be doubtful whether we are confronted with real thinking, with fixated thoughts, or even only with words.

The most important function of thinking is the unifying activity by which various phenomena that owe their existence to similar previous procedures are put together in a unit, the unit of a concept. Concepts or parts of them are combined into thoughts; higher thoughts are built through the process of syllogism.

In the beginning of the thinking process we are more or less focussed in a special direction in which thinking should proceed. The further procedure does not consist only of bringing out the right thoughts, that is, those suited to fulfill the task, but also of the elimination of what William James has called the fringes that cluster around the phenomena in the normal stream of consciousness. This selection is due to the capacity of abstraction by which the phenomena in the fringes are eliminated so far as they disturb the process of thinking.

Concerning the relationship of thinking and speaking, I remind you that the patients who have lost the capacity to use words as symbols show the same deficiency in thinking. The results of examination by the mentioned tests that do not demand language for their execution revealed that the nonlanguage behavior of the patients is also deprived of the influence of abstraction and has become more concrete. The patients fail in all every day behavior similar to the tests, in all conditions where a task can be fulfilled only with the use of the abstract attitude, while their behavior is in principle like that of normals when only concrete activity is necessary. We find the same disturbances in patients with damage to that part of the brain cortex that we consider important for the nonlanguage function as in that part important in thinking.

A survey of the failures of the patients in all fields of behavior has allowed us to determine various modes of behavior that demand an abstract attitude. I mention the following that can be considered characteristics for abstract behavior and also for thinking:[7] to assume a mental set; to shift reflectively from one aspect of a situation to another one; to hold in mind simultaneously various aspects; to grasp the essential of a given whole; to break up the whole into parts, isolate, and syntheize them; to select common properties reflectively; to form concepts; to detach the ego from the outer world or from inner experiences; to distinguish what we call objects, fantasies, and images; to account

to oneself for acts; to plan ahead ideationally; and to think or perform in symbols. Many of these modes may appear basically the same although occurring in different situations. In our attempt to find a common denominator, we came to the conclusion that the patient is basically disturbed in the attitude toward the merely possible, in other words, in the category of possibility. As a result of these findings, the category of possibility appears most characteristic for human behavior. In respect to our problem it is vital for thinking. Thus to understand thinking we have to be aware of whether the situation in which it occurs demands behavior in one of these modes. The particular significance that the category of possibility has in thinking makes it apparent that thinking can never be absolutely correct (as demonstrated later).

In the transformation of thinking to language, the organization of the thoughts is modified according to the linguistic character of the particular language. One word does not correspond to each member of the thought process; sometimes several thoughts are represented by one word or even a part of a word. Sometimes one thought can be expressed only by several words or even more complex speech configurations. In this transposition speech automatisms play a particular role.

The linguistic character of the various languages shows in different speech instrumentalities, differing to such a degree that the translation from one language to another one is not only a very difficult job but can usually be performed only in an imperfect way. Much misunderstanding in verbal communication is based on this difficulty. It should not be forgotten that, by these differences in languages, thinking also is influenced.

Sometimes when we begin with communication of a definite idea as, for example, in a lecture, and have already expressed this idea in language, we may feel that we cannot proceed further in the same direction; then we may stop and begin anew, but we can also modify the further construction of our presentation. When speaking and the process of thinking seems to cease, the speaker may repeat the preceding sentence without even being aware of it and may then continue quite well. In patients we very often see such repetition if they are not able to fulfill a task, but they may continue if they subsequently are confronted with a task that they can fulfill. Repetition thus appears to be a kind of protection against the danger of not being able to fulfill a task.

We can call the order of thinking the grammar of thinking. It is different from the grammar of speech. The disturbance of each finds its expression in different modifications of the grammatical and syntactical forms. Concerning the very interesting disturbances of grammar, referring particularly to the publications of Pick[14] and myself,[4] I shall here make some remarks about the so-called telegram style of patients with motor aphasia. Even if the patient has regained a great number of words, which he could not speak due to a defect in the motor sphere of language, he omits in spontaneous speech definite word categories; the prepositions, the articles, and the grammatical forms are missing while nouns, adjectives, proper nouns, and verbs are present, the latter two categories even prevailing. There are no disturbances of the syntactical structure. That the patient has not lost the words that are missing in his presentation shows in that he can produce them in repetition, even if often with motor

defects. The patient apparently restricts his speech—due to his general motor difficulty in speaking—to those words that are sufficient for him to be understood.

We observe a similar phenomenon in the difficulty in pronouncing words in a foreign language. We omit more or less voluntarily all words that we believe are not necessary to be understood. It is not quite correct to describe this procedure as omission; more correct would be to say that the speaker is concentrating on the thought he wishes to express and brings that thought, in words, to the fore. Thus a number of superfluous words fall out. The speech of these patients remains understandable because the syntactical order of words and the correct selection of the important ones is directed by their intact thinking. Thus we observe this agrammatism particularly in patients with normal abstract behavior.

In patients with impairment of abstraction we find particularly the small words missing,[4] chiefly the articles, the pronouns, the prepositions, the inflections, and the conjunctions, that is, those small words that are customarily used in various combinations under special conditions, for example, in an address on an envelope. These patients may be able to use the words in such usual combinations but not individually; they are not able to speak them, to write them, to read them, or to repeat them. These words presented in isolation are strange to them and do not mean anything to them. This shows that the isolated presentation of these words can be grasped only by abstraction. Failure to understand these small words when spoken isolatedly may reveal itself in a very peculiar way. For example, when the patient hears the sound "for" that is similar to the sound of the word "four", he may be able to understand the sound only in its numerical context, assuming this is appropriate to the situation in which it is presented. However, the patient will not be able to react in any way if the sound presented does not, for him, belong to an experienced, concrete situation. Therefore, the sound of the word "for" used by us as a preposition is totally without meaning for him and, as a consequence, he cannot do anything with it. He cannot copy it, he cannot read it, and he cannot speak it or repeat it.

I shall here finally make some remarks about the relationship of repetition of language to thinking. Repetition is not at all a simple automatic performance as is often assumed. It depends upon different conditions whether the sound complex is simply repeated or there is an attempt to understand it before the repetition. The level of cultural status and the inborn gift for language and its development play a significant part. Educated individuals are more inclined to reproduce exactly the sound complex presented to them. Less educated persons may not be able to do that upon request; they try first to understand what the word may mean and then repeat the sound complex in their own idiom. In other words, people of lesser education react more with the whole personality, that is, in a more concrete way; they are less able to consider the presentation and their reaction as an isolated phenomenon because that may be senseless to them; it requires some abstraction that they are not accustomed to apply to such a situation. This manifests itself in patients with impairment of abstraction by their inability to repeat combinations of senseless syllables.

Another phenomenon concerning the relationship of repetition and thinking is that instead of the words that he cannot produce due to impairment of abstraction, the patient produces other words that belong to the sphere of meaning of the demanded words. For example, asked to repeat a word or to name an object, such patients utter instead of the word "God" the word "church," instead of the word "house," the word "village," and vice versa. I refer in respect to these phenomena to an important investigation of this problem by Lotman.[13]

The analysis of the substitutes appearing in this defect of thinking teaches us much about the deviation of language under difficult conditions of normal life, when the individual is not equal to the situation with thinking and speaking. That occurs, for example, in states of anxiety when abstracting capacity is diminished and the individual is reduced to the use of concrete associative behavior. We may understand the abnormalities here in the same way as we do in patients when the total situation in which they occur is known. They may be determined by an extraordinary situation or by ideas that we do not know or we are not able to comprehend. Only by taking into consideration all these factors shall we do justice to the individual and avoid wrong interpretations. Examples of this can be found in the thinking and speaking behavior of less educated and so-called primitive people as well as in the expressions of highly educated and exceptionally gifted persons, such as philosophers, who try to communicate new ideas for which the usual thoughts and words are not sufficient and, further, particularly in the language of poets.

In defect of thinking in patients, speech automatisms belonging to definite thoughts or their fringes come abnormally to the fore. The picture can become especially complicated when words belonging to the fringes of the substituted words force themselves into the process. We are then confronted with the picture of different kinds of disturbances in thinking and speaking. There is one such disorder that is usually called a flight of ideas. Sometimes we find severe confusion. It is interesting that even then the disorder can become understandable when considered from the point of view that it represents different ways of substitution and that the result is an expression of the patient's attempt to escape the danger arising from the impossibility to cope, in his abnormal condition, with the demands of the situation.

It is particularly interesting to note those instances in which a patient with a deficiency in the sphere of thinking substitutes for his defect not only by language but by definite movements of the body—which are for him associated with the words—and uses these activities in the process of making a syllogism. A patient with defective thinking was asked to construct a conclusion from two premises. He was not able to do that. He seemed not even to understand what we wanted from him. However he repeated the two sentences again and again and finally, by substitution, came to a result that at face value seemed correct. The following sentences were presented to him: "Oil swims on water. Castor oil is an oil." The normal answer would be that Castor oil is an oil and so it swims on water. How did the patient proceed? Repeating the sentence "Oil swims on water" several times he accompanied the speaking with movements of his outstretched arms similar to the swimming movements in water. Then he repeated "Castor oil is an oil" accompanying that with the

same swimming movements. Then he repeated "Castor oil is an oil" making the swimming movements and rapidly ejected the words "Castor oil swims on water." How much that represents a concrete behavior based on passively originating associations shows in the fact that the patient was not able to understand what the sentences meant. He was also apparently not aware of his acting and was far from understanding the process of a syllogism.

The substitutions are interesting from still another point of view, namely, that they may be misleading in our interpretation of the performance capacities of the individual.

There are patients whose phenomena, appearing as substitutions for the defect of the symbolic character of thinking and speaking, show peculiarities that can simulate high forms of behavior. If the sentences are undisturbed in rhythm and pronunciation of the words because the instrumentalities are intact, they thereby give the impression of a normal way of speaking. This may be intensified when the patient uses words or even sentences that normally belong to the language of a particularly intelligent and gifted person. Such observations have induced the assumption: "The brain injury can bring to the fore elements in the nature of the individual which disease could not destroy but releases. . . . Disease may occasionally even reveal—though in a distorted fashion—what is great and noble in man's nature."[16]

That may be true in so far as the utterance is that of an originally high-level personality. What the patient utters, however, is not the expression of such a high symbolic attitude but only the preserved instrumentalities previously belonging to the former that is lost by the damage of the brain. In any case we should be cautious in the interpretation of the phenomena. We may have to deal with pseudosymbolic performance. The thinking and speaking of schizophrenics have often been misinterpreted in this way. The situation in this disease is particularly complex because the patients may, in some conditions, show a definite lack of abstraction, symbolic thinking, and speaking, but not in others, where their thinking and speaking have symbolic character. Without going into the extensive ramifications of this, I stress that what occurs depends in a systematic way on the total situation. In this light these patients present particularly interesting material for studies of the character of thinking and speaking in relation to the whole personality and the influence of special situations on the use of abstract and concrete behavior in general and so also in thinking and speaking.[10,12]

I am aware that, in my discussion of the problem of thinking and speaking, I could not do more (as I said before) than to explain the general point of view from which—in my opinion—it can be dealt with fruitfully and to demonstrate my procedure with some examples. I had to forego many important aspects of the problem. I mention in this respect particularly the inner speech that is of great significance for our understanding of the relationship between thinking and speaking and to the clarification of which the material presented by the patients has contributed much.[4] For a fuller presentation of this and other problems, I refer to a publication to which a number of experts in the field contributed.[17]

I shall now recapitulate the major conclusions of my study in the following summary.

Thinking and speaking are, as all organismic performances, understandable only when they are considered in relation to the total personality of the speaker and the task with which he is confronted in the given situation. They represent man's specific means to fulfill the general basic organismic trend to use all the possibilities to the greatest extent under the given condition; they are the specific means of man to guarantee all his existence.

Thinking and speaking each follows, corresponding to each structure, its own definite laws that, however, become effective in different ways according to the respective demands of the whole organism and the situation. Both are instigated by the different occurrences in the organism in its attempt to come to terms with the outer and inner world, although thinking in its symbolic form plays the dominant and regulative role.

We have to distinguish in both realms the function proper that has the character of symbolic interpretation and organization of the world and the "instrumentalities" by which organization is actualized. According to the situation, that is, whether self-realization can best be attained by the one or the other mode of thinking and speaking, the one or the other comes into the foreground "figure." However, what is executed stays always in relationship to the "background" that is built by the activity of the other modes of thinking and speaking and the general condition of the whole individual.

We can say thinking and speaking have essentially the same goal. The difference between the performances consists of the difference in the means by which the goal is accomplished. The identity of the goal makes it understandable that neither of them can come to full development independently and that disturbance of one always manifests itself more or less in modification of the other. Isolation of one from the rest of the organism—as happens under abnormal conditions in normal life as well as in pathology—changes the normal relationship between thinking and speaking in such a way that one or the other becomes predominantly modified whereby the symbolic character particularly is diminished, and automatized thoughts and words come to the fore and easily simulate thinking proper and meaningful speech.

When it is stressed that during thinking speech processes of the character of automatized instrumentalities become effective, this should not lead us to assume that the main purpose of speech is to fixate thoughts, a conception that, for instance, was expressed in Arthur Schopenhauer's words: "That the true life of a thought lasts only so long as it has arrived at the frontier of the words. Then it is petrified, is dead from then on." This certainly is correct only to a limited degree and under certain conditions. In general it must be said that speech is a very important means to enrich thought—particularly due to its symbolic function—or, expressed from our point of view, speech is a precious means to bring the personality and its world to a fuller development and thus to a higher degree of self-realization. W. von Humboldt and E. Cassirer have in particular stressed this fertilizing character of speech. Even if speech, according to Humboldt, restrains thinking to a certain degree, "on the other hand the word which makes an idea individual in the world of thoughts adds to it much of its own; each important language is a particular means to produce and communicate ideas." The same concept found its expression in the words of the philosopher Max Scheler when he wrote: "As much as language imprisons

life in domineering schemata it enriches it in new forms of expressions and liberates it so from the dreadful muteness of our inner experiences."

There is scarcely a better example to show the significance of speech than the observation of our patients. The personality of the patient and his world are greatly restricted; the patient is deprived of the characteristic of individuality. He no longer experiences an ordered world in which the objects are related to each other in a definite way so that it may be anticipated with some certainty what will happen; he is confronted again and again with new situations and things. With that goes along a deep change of the possibilities of action in the normal human world. It could even be doubted whether we have the right to speak of a "world" at all in this situation.

It is true that language brings into life not only order but also ambiguity. We have repeatedly referred to the fact that language may easily feign thinking and can deceive us. That has evoked again and again—in discussion of the problem of langage by Greek sophists—a skeptical attitude toward thinking and speaking, particularly the latter as a trustworthy means of communication. Concerning this skepticism, I shall make a few remarks in relation to the concept of the nature of man to which I came from my inquiries. It was said before that the patients are unable to say, to do, or to think of something that is merely possible, that they are unable, therefore, to make any choice, and also that this is related to a fundamental change in their natures. This is understandable, because this capacity is the basis of man's freedom, his essential distinction from all other living beings. The paramount significance of this capacity of possibility should not let us overlook the danger it brings: where there is freedom of choice, there is possibility of failure.

If we, in our comparison of the behavior of the patients with that of normals, consider not only the defects but also what the patients are able to do, then we make the amazing observation that what they are able to do they perform with remarkable exactness, sometimes even greater than normal. That is the effect of the incapacity to make a choice, of the absence of the category of possibility. The ability to make a choice is not only a source of error but, because choice is dependent upon an individual's decision, there is also the possibility of intentional misrepresentation. This applies to behavior in general but particularly to speaking. Therefore, in all human communication there is a risk. We may avoid this more or less if we consider each utterance from the viewpoint of the total condition in which it occurs, which indeed has to include particularly the intimate knowledge of the personality of the speaker. Thus confidence in his integrity that is based upon a real communion between speaker and listener[11] becomes paramount, in my conviction, the presupposition of human existence in general. This is the case of true evaluation of speech.

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Part II. Developmental Problems of the Psychology of Thinking

LEARNING, DEVELOPMENT, AND THINKING

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Many have remarked about the relative quiet that preceded the modern storm of interest in the psychology of thinking. Among the behaviorists the period of quiet was premeditated. The earliest published polemic of Watson on behaviorism holds a surprising admission for one so intrepid. After a vigorous exposition of the virtues of a behavioristic approach he admits, with reluctance, that the situation is somewhat different when we come to a study of the more complex forms of behavior, such as imagination, judgment, reasoning, and conception. "Our minds," he said, "have been so warped by the fifty-odd years which have been devoted to the study of states of consciousness that we can envisage these problems only in one way. We should meet the situation squarely and say that we are not able to carry forward investigations along all of these lines by the behavior methods which are in use at the present time." Of course, he quickly points out that the introspective method had already reached a *cul-de-sac*. These problems would be better put aside until they can be viewed as they arise from a fresh point-of-view and in more concrete settings.

Nevertheless, at about the same time, Hunter formulated a concrete setting to study higher mental processes. I refer, of course, to his well-known delayed reaction studies. Consideration of the results he obtained led him to recognize the inadequacy of the simple S-R paradigm for dealing with such behavior. In 1924, he formulated an approach that had much in common with modern mediated response theories. However, in spite of these accomplishments, he "read the signs of the times" in the same way as Watson. He predicted that ". . . work in this area will become less and less dominant in the science until a thoroughgoing attack can be made upon it in the light of methods and results derived from work upon simpler but otherwise comparable processes."

By 1920 Hull had already published an extensive study on thinking. It was characteristic of him to develop a quantitative method to study concept formation. Although I know of no prophesies that he made, he too deserted this area of investigation in favor of work on less complex processes.

There were eminent psychologists like Wertheimer (1943) and Duncker (1935) who continued to investigate thinking, but the behaviorists for the most part left the field to concentrate on developing a better understanding of the phenomena of simple learning. For several decades, true to the forecast of Watson and Hunter, research on problem-solving by United States behaviorists was virtually suspended. Recently, however, workers have displayed a reawakening interest. There has been an upsurge of experimentation in problem-solving behavior. Whether this renascence is related to the progress made in accounting for simple learning or to a subtle kind of public demand I am not

certain, but the literature in this area seems to be increasing at an accelerating rate.

Two trends that are worthy of analysis have accompanied the rising interest of the neo-behaviorists in the study of complex processes. One trend, that I shall discuss presently, is the increasing reliance for explanation on implicit covert mechanisms presumed to mediate between the external stimulus and the overt response. The other trend is the new interest in the developmental process.

Although the study of development has been the focus of some of Europe's most distinguished psychologists, for example, Jean Piaget and A. R. Luria, it is viewed with suspicion, or at least with reservation, by some behaviorists in the United States.

One explanation offered for their wariness is that assumptions about innate processes have unfortunate historical associations, having so often served as covers for teleological pseudo-explanation, metaphysical vitalism, theism, and the like (Campbell, 1959). Because behaviorism is very preoccupied with establishing psychology as a respectable science, the movement has been careful to avoid contamination with a doctrine so tainted.

Then there is the tendency for areas of special interest in psychology to regard themselves as the hub of the science. For the behaviorists, learning is all.

These are trivial reasons. With the increasing maturity of scientific psychology such deterrents to research on developmental processes will continue to dwindle to their proper level of insignificance.

There is, however, another set of considerations that are not so lightly dismissed. Behavioristic learning theory is committed to studying the processes that relate the organism to its environment through its past history. It has produced a growing, vigorous science that studies the effect of experimental manipulations of three classes of variables on behavior: motivation, reinforcement, and stimulation. There is an implicit assumption common to all learning psychologists that the processes uncovered by experimental investigations are common to all organisms. It is presumed that there is a generality to the laws that relate the organism to his environment that spreads across phylogenetic and ontogenetic divisions and that there is a commonality to the learning process from its simplest to its most complex manifestations.

Learning psychologists are thus freed to study one convenient laboratory organism, primarily the rat or the pigeon, bolstered by the assumption that their findings are not limited to the species studied. The emphasis is on generating laws of behavior. Phylogenetic and ontogenetic comparisons are considered mildly interesting but not basic. Where human beings serve as subjects, the emphasis is on demonstrating the universality of the behavioral laws derived from animal experiments. The similarities are more important than the differences.

As long as behaviorists deal with simple processes, concentration on one organism like the pigeon, in one kind of experimental situation like the Skinner box, has immense advantages. It provides a stable methodology, reduces problems of communication between experimenters, and increases experimental control. However, when we become concerned with "thinking" we find we

must leave these safe shores and grapple with the complexities presented by differences between species and differences between age levels.

Does this departure imply a break with the assumption of generality? Not necessarily. It could strengthen the position with empirical demonstration. For the past several years Howard H. Kendler and our associates and I have been engaged in research that applies a comparative-developmental approach to the study of concept-formation. We have been as much, or perhaps more, interested in the differences between species and age levels as in the similarities.

FIGURE 1. A paradigm of the successive discriminations in reversal and nonreversal shifts.

Nevertheless our theorizing has been, to date, entirely within the S-R behaviorist tradition.

The program began with an experiment on concept-formation in college students (Kendler and D'Amato, 1955) that used a technique that was later adapted to each of the experiments I will describe today. Since the procedure is complex and each study involved a minor modification, it will be easier to explain and to understand if I present a paradigm of the technique at the outset. I say paradigm because it is not actually identical to any of the procedures but conveys the essence of all of them.

The procedure that is illustrated in FIGURE 1 consists essentially of two successive discriminations between pairs of stimuli. The first discrimination differs simultaneously on two dimensions; size and brightness. *S* is rewarded for responses to one dimension; the other dimension is irrelevant. For example,

S has to learn to choose the large cup, regardless of brightness. After reaching criterion he is shifted, without warning, to a new pattern of reinforcement. If he is a reversal shift *S*, he is required to respond to the same dimension on which he was originally trained, but his overt choice has to be reversed, for example, he has to shift from a large cup to a small one. For a nonreversal shift *S*, the previously irrelevant dimension becomes relevant, for example he has to shift from large to black.

Comparison between these two types of shifts is of particular interest because analyses of the acquisition process based on mediated and unmediated S-R connections yield different and easily tested predictions about their relative efficiency. FIGURE 2 presents a schematic diagram of both types of connections.

The single unit formulation would predict that reversal shift would be learned more slowly than the nonreversal shift. This would follow because, at the time of the shift, the difference between the strength of the dominant incorrect

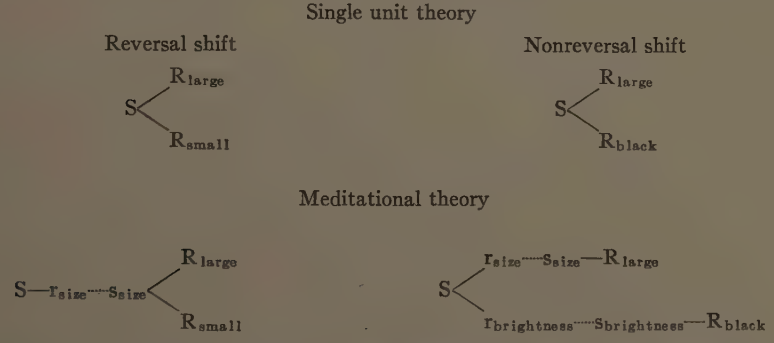

FIGURE 2. A diagram of the competition between the habits during reversal and nonreversal shift according to the single unit and mediational theories.

habit and the to-be-correct habit is much greater for the reversal than for the nonreversal shift. It should therefore be more difficult to learn to respond to the previously relevant stimuli in an opposite manner than to respond to a stimulus that was previously irrelevant.

To check the prediction, Kelleher (1956) applied the reversal-nonreversal shift technique to rats. His results confirmed the single unit theory. For rats, a reversal shift was more difficult than a nonreversal shift.

First Buss (1953) and a little later Kendler and D'Amato (1955) used college students as subjects in a more complex, concept-formation variation of the reversal and nonreversal shifts. Both investigations reported that, unlike the rats, the students performed the reversal shift more rapidly than the nonreversal shift.*

* Buss (1953) initially attributed the slowness of the nonreversal shift to the fortuitous intermittent reinforcement of the previously correct concept under this condition. All of the research comparing these two types of shifts cited in this paper, with the exception of the 1953 Buss study, used procedures that eliminated such intermittent reinforcement. In 1956 Buss compared reversal and nonreversal shift with this factor eliminated. Under these conditions he also found that for college students reversal shift occurred more readily than nonreversal shift.

It may be unexpected to the reader, but the difference between rats and students was no surprise even to behavior theorists. As early as 1930 Hull had pointed the way for behavior theory to extend its range by introducing the concept of the pure stimulus act. This "act" or, as it is more frequently called, cue-producing response may be overt or it may be covert. Either way it is assumed to give rise to stimuli that can, like an environmental stimulus, become associated with an overt response thus enabling an organism to generate his own cues. These cues, in turn, serve to guide his behavior.

Kendler and D'Amato applied a mediational analysis to predict that reversal shift would be relatively easy for college students. As illustrated in the bottom half of FIGURE 2, reversal shift requires *S* to utilize the same mediated response that he has previously employed with success. Only the overt response must be changed. A nonreversal shift, on the other hand, requires the acquisition of a new mediated response, the cues of which must become attached to a new overt response. Consequently the reversal shift, which requires a change in only the last link, should yield faster learning than the nonreversal shift.

This discontinuity between the behavior of college students and white rats directed our attention to the psychological processes responsible for the *development* of mediational responses. It has been our plan to use a comparative-developmental approach to discover first if, and later how, the child makes a transition from a single unit S-R association to a mediational mechanism. Today I will report on some studies directed toward this end. However before presenting the hard data, I will say a few words about mediational mechanisms.

All theories of thinking, motor or central, behaviorist or phenomenological, postulate internal processes that intervene between the problem and the solution or the stimulus and the response. The behaviorists have historically been identified with motor or peripheral theories. Watson (1914) for example, described thinking as subvocal talking. The verification of his position lay in direct measurement of the muscles of speech thus making the intervening process directly measurable behavior. Phenomenologists view the central process in terms of conscious experience that is directly described by the report of the subject. These two positions differ in what they consider the basic datum; they are similar in their efforts to provide direct measurement of the mediating thought processes.

There is another approach to the mediating process that is associated with the neobehaviorism of Hull and is exemplified by our research. It does not depend on making the internal process directly observable. Instead guesses or assumptions are made about what happens inside the "black box." Hypotheses are generated from the assumptions and experimental verification is sought. No hypothesis achieves justification or validity prior to empirical testing, and no other procedure provides acceptable verification. The assumptions may be as intuitively acceptable as those derived from phenomenological introspection or as abstract as those generated from cybernetics. Our own pretheoretical model happens to derive from learning theory. Regardless of its beginning, the origin of the hypothesis does not endow it with any validity prior to the empirical test.

The empirical test consists of the ability of the hypothesis to predict the

response of an organism as a result of specified variations of experimentally manipulated variables. The independent variables are described in physical terms and the ultimate response of the organism must be publicly observable. Separate operations must be available for the description or measurement of the independent and dependent variables lest the relationship be reduced to a tautology. Verbalization of the *S* is acceptable datum, but they are not necessarily considered as direct measures of the intervening processes.

The basic assumption we have made in our research is that thinking is amenable to a behavioral analysis, that it can be described in S-R behavior theory terms. The college-student study I have just described, as well as a host of studies by other investigators, requires that we further assume that thinking involves chains of S-R units. To meet the requirements previously set forth, the stimulus for the first link is measured in physical terms. The intervening links may or may not be directly observable, but the end product must be. The links, since they are S-R units, obey the laws applicable to S-R connections, for example they can be strengthened by the application or extinguished by the withdrawal of reinforcement.

One immensely important set of responses that serve to differentiate the human being from the rat are language responses. Adult human behavior is the product of a considerable history of reinforcement prior to any particular research even in a carefully controlled laboratory experiment. The subject enters the situation with a wide repertoire of verbal responses. These responses produce stimuli that govern his overt response. Unlike the rat in the Kelleher experiment, the behavior of the adult human is not as much governed by the external cues presented. He is also guided by a system of cues that he generates himself.

Thinking along this line naturally leads the investigator toward the study of when and how such mediating responses develop. For example, one question that could be asked is whether there is a stage in the development of children at which they respond according to the single unit theory, that is, when they are more like the rats than the college students.

To answer this question we adapted the reversal-nonreversal shift procedure to children. Our first *S*s were kindergarten children between the ages of 5 and 6, some of whom were presented with reversal shifts and some with nonreversal shifts (Kendler & Kendler, 1959). Remember that a single unit theory that was capable of predicting the behavior of the rat in a comparable situation predicts that reversal should be slower than nonreversal. The mediational theory predicts that reversal should be faster than nonreversal. When these shifts were compared for the kindergartners the results revealed no statistically significant differences at all, confirming neither prediction.

One conclusion that we could draw is that children are a special kind of organism with laws unto themselves to which neither theory applies—a very tempting conclusion for a parent. However there is another interesting possibility. The failure to achieve results consistent with either those obtained from rats or from college students may have been due to the fact that these children are in a transitional stage of development, in which these tasks lead some to function on a single unit S-R basis while others are making relevant

mediated responses. If these two groups were about evenly divided they would yield total results such as we obtained.

No MA scores were available, but there was a measure that was perhaps more relevant to the task in hand, that is, the number of trials to criterion in the training discrimination. *S*s were sorted into two groups on the basis of their initial performance on each dimension. Those scoring above the median, that is, took more trials to learn, were labeled slow learners. *S*s scoring below the median were labeled fast learners. The results of fast and slow learners were analyzed separately.

Of course, we expected the slow learners to be more like Kelleher's rats, and fast learners to be more like college students. The results shown in TABLE 1 confirmed these expectations. Analysis of variance indicated that the predicted interaction was significant at the 0.05 level.

TABLE 1

MEAN NUMBER OF TRIALS TO CRITERION ON TEST DISCRIMINATION FOR *S*S SCORING ABOVE AND BELOW THE MEDIAN ON THE TRAINING DISCRIMINATION

Group	Performance on training discrimination	
	Above median (slow learners)	Below median (fast learners)
Reversal	24.4	6.0
Nonreversal	9.0	15.5
Control	7.3	6.8
Combined	13.6	9.4

It therefore seemed reasonable to conclude that these children, taken as a group, were in the process of developing mediating responses relevant to this task and that some were further along than others.

If this interpretation is correct it should follow that in a group of still younger (that is, preschool) children who worked with the same concepts, a smaller proportion should have developed appropriate mediating responses. A second experiment (Kendler *et al.*, 1960) was done on nursery school children whose ages ranged from 33 to 63 months with a mean of 48 months. It would be expected that such a group would show more negative transfer on reversal shift than on nonreversal shift. The children performed according to prediction.

At present we are nearing the completion of a study that allows to the *S*s one of three possible bases for making a shift from a first to a second discrimination. The shift may be on a reversal basis, on a nonreversal basis, or on a third basis called inconsistent, which I shall explain presently. All *S*s learn the same discriminations. It is the *S*s in this procedure and not the experimental manipulations that, so to speak, "choose" the basis on which the shift is made. The procedure, which is too complex to describe here, makes it possible for us to determine the basis of the shift without relying on introspection. The *S*s of this experiment were children of 3, 4, 6, 8, and 10 years of age. Our purpose

was to see whether the proportion of children who make mediated responses, as represented by reversal shifts, increases in a lawful and regular way with age.

To date 123 of the projected 160 children have been tested. The results thus far show that the percentage of children who respond in a reversal basis is 25 per cent at age 3 and that this proportion rises gradually to 62 per cent by age 10. Incidentally, Buss (1956), using a comparable procedure, found that 72 per cent of college students responded on a reversal basis, a result that is quite congruent with those presented here. It is a heartening corroboration of the stability of these results to note that it is at age 6 that exactly 50 per cent of the children are reversing. This age is close to the mean age of the kindergarten group in our first study with children. It was this group that we divided in half on the assumption that one half of the *S*s were mediating and one half were not.

The results also show that the proportion of children who respond inconsistently decreases with age, from 50 per cent at age 3 to 10 per cent at age 10. This trend is also consistent with our formulation. When a nonmediating child is faced with a choice between responding on a reversal or a nonreversal shift basis, he should favor the latter for reasons already explained. If learning is rapid, he should be categorized as a nonreversal shift. However, a child who does not mediate is responding nonselectively. All stimuli impinging on him at the time he makes a rewarded response become associated with that response. Therefore if the learning is long, and if, as in our situation responses to "reversal and nonreversal stimuli" are rewarded simultaneously, the favored status of the nonreversal stimulus should ultimately disappear. When faced, with a choice, such a child should respond part of the time to each stimulus, inconsistently. We would therefore expect exactly what we found, that is, young children, since they are predominantly nonmediators, would show the most inconsistency and the proportion should decrease with age. We would also expect that the inconsistent category would be the slowest learning group, the reversal group should be the fastest, and the nonreversal should fall between the others. Based on the incomplete returns, the median trials to criterion for the second discrimination are 2.5 for reversals, 6.5 for nonreversals, and 18.5 for inconsistents.

The percentage of nonreversal choices seem to be unaffected by age. I must confess that I cannot yet explain this result.

On the whole, it is safe to say that the results so far suggest that we are pursuing a developmental process we call the development of a mediating response. This process seems both amenable to measurement and analysis and, furthermore, appears to be almost surprisingly lawful.

Recently we have also turned our attention to how, rather than when, mediation develops. Since language is so important, we began with the manipulation of the overt language behavior. Even the youngest children we worked with could speak. We demonstrated in one of our previously described experiments that it is quite feasible by means of instructions to get nursery school children to make overt verbal responses that apply to the situation. Moreover, they easily learn to interpose these verbal comments between the presentation of the stimulus and the overt, physical response. If their ultimate

behavior is not influenced by mediating verbalization, it is not because they cannot do it. It is either that they do not make the response or that they make the response but it does not mediate.

Perhaps there is a stage in development in which verbal responses, though available, do not mediate readily between external stimuli and overt responses but rather form a parallel connection with little or no overlapping effect. This idea was suggested by the observation of several children who spontaneously verbalized the correct solution while they simultaneously chose the wrong stimulus. Luria (1957), the Soviet psychologist, made a similar observation on the basis of his work with children. He states: "In the early stages of child development, speech is only a means of communication with adults and other children, whereby the child masters in a generalized form the experiences of other people. Subsequently it becomes also a means whereby he organizes his own experiences and regulates his own actions. So the child's activity is mediated through words." If this is the case, supplying very young children with relevant verbal responses will not influence their reversal shift behavior.

The alternative possibility is that the age variable is influential only in so far as it leads to the production of the verbal response and its accompanying cues. If a relevant mediation is supplied, age differences will diminish or disappear.

To explore these issues, children of four and seven years of age were presented with another variation of the reversal shift problem as shown in FIGURE 3. They initially learned a simple discrimination between one pair of stimuli that differed simultaneously in size and brightness. In the illustration provided in the figure, the large black square is correct. While they were learning, the children were required to describe aloud the stimuli to which they were responding. One third learned to say large (or small as the case may be) by the simple device of asking them to tell us which was correct, the large or the small one. Another third learned to say black (or white) in a corresponding way. The remaining third was not required to say anything. After learning the discrimination, all *S*s were presented with a reversal shift. In our example the shift is to small, regardless of size. Thus the group that initially described the correct stimulus as "large" had verbalized the relevant dimension. The verbal response of "black" was irrelevant to this reversal shift.

Before we consider the age differences it is pertinent that for both age groups combined relevant verbalization was better and irrelevant verbalization was poorer than no verbalization. In general then, this particular form of verbalization does serve to mediate behavior, and its influence takes the expected direction.

Having demonstrated that we had an effective, experimentally manipulatable, mediating response, we could address ourselves to the developmental question. Does the utilization of this particular form of potential mediation differ with age? FIGURE 4 shows the results of the three experimental groups for the two age levels.

If developmental processes affect the utilization of verbal responses in problem solving, then it would be expected that our three verbalization conditions would affect the two age groups differently. The results suggest that there is an age effect, but they lack the finality that is provided by statistical signifi-

cance. The interaction mean square provided by a rather complex analysis of variance falls short of the 0.05 level of confidence. Nevertheless, I shall take the liberty to point out some of the obtained age differences, if only as a guide to further research.

It can be seen that the younger children were influenced by verbalization; they profited by learning relevant verbalization and were hindered by learning irrelevant verbalization, but the effect was small. Although the relevant verbalization reduced the difference between the ages as compared with no verbalization, there was still some difference in favor of the seven-year olds.

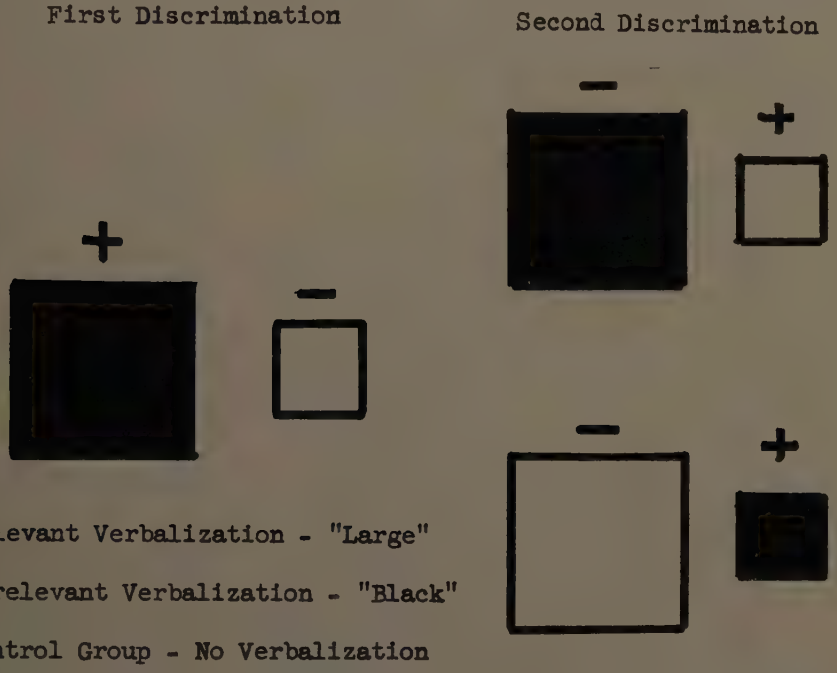

FIGURE 3. Experimental procedure to study the influence of verbal labels on a reversa shift.

As for the older children, supplying relevant overt verbalization did not produce any improvement over no verbalization, but irrelevant verbalization yielded a marked interference. This effect is so potent that in this condition the performance of the seven-year olds is slightly worse than the younger children.

What these results suggest is that younger children are less responsive to their own verbalization than older children. Getting young children to say things that are relevant to the problem in hand appears thus far to yield no guarantee that they will utilize their own words. Older children seem more likely to be influenced by what they say.

In general terms, providing a relevant mediating response does not guarantee its use in a problem-solving situation. Some interaction is required before

this response mediates or provides cues that influence behavior. We do not know yet what the necessary conditions for this interaction are, but age or level of development seems to make an important contribution. There remains much to be done before we can pin it down further.

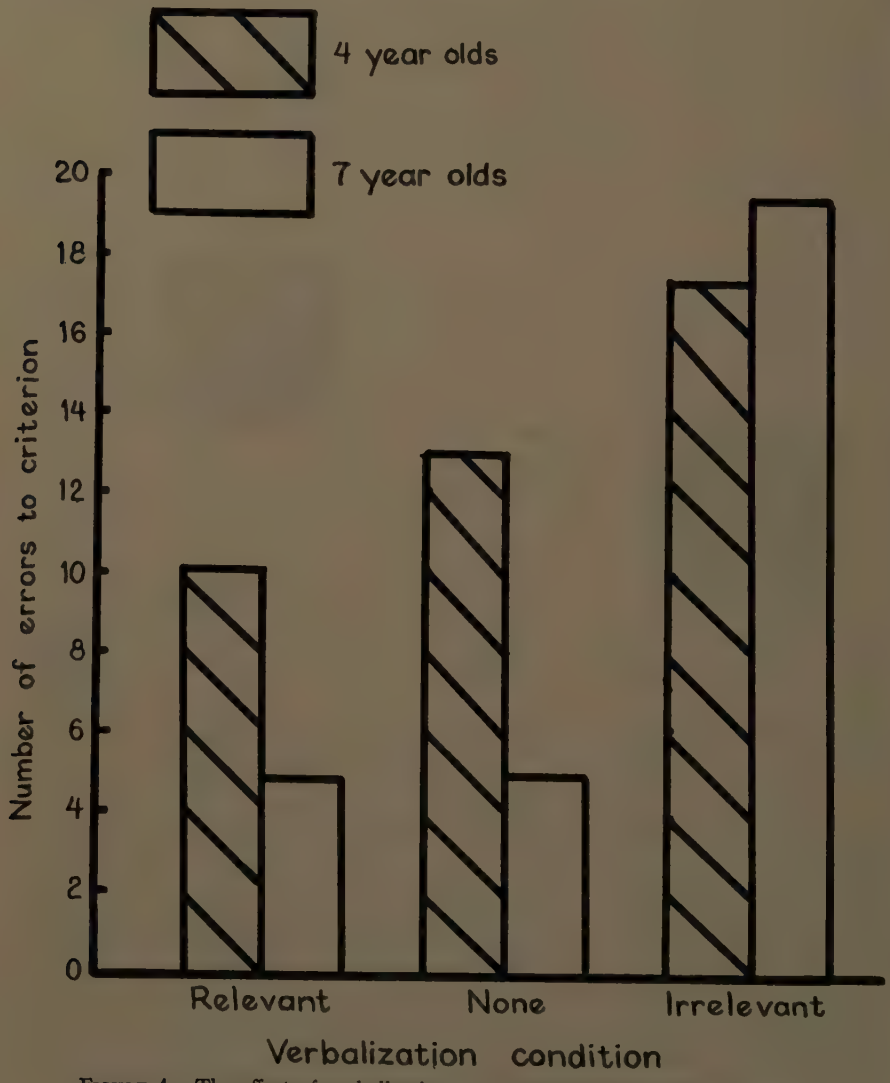

FIGURE 4. The effect of verbalization on reversal shift for two age groups.

That we have initially chosen to investigate the potentialities of language should not imply that we believe all mediation to be verbal. As Hull (1930) saw it, the pure stimulus act is not synonymous with, but is more fundamental than, Watson's subvocal response. It is quite possible for responses other

than vocal to serve as symbols. It may be, as suggested by Werner and Kaplan (1957) and Piaget (1952), that in the developmental process, a gestural or postural mediation precedes and perhaps lays the basis for later verbal control. We plan to continue our investigation by studying many kinds of potential mediators, both as to how they affect the thinking process and how they are in turn affected by the developmental process.

In summary, I have described a research program that is concerned with the development of mediating responses, a process which we consider fundamental to thinking. The program derives both its theoretical model and its research procedures from learning theory. Our procedure is highly analytical; it proceeds in depth rather than in breadth. It is concerned with adequate measurement, clear definitions, and lawful relations. It is also concerned with extending the generality of the learning principles derived from simple learning situations, but we are prepared to add new variables that measure the developmental contribution if and when that seems necessary.

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Discussion of the Paper

Question: I was curious as to whether you had speculated at all about this six-year switch in the use of the mediating response: was it a matter of neurological maturation or learning experience? This immediately raises the ques-

tion of what kind of children you were dealing with. I assume that you were dealing with middle-class children from a college setting. In such a group you would expect this mediating response to be more widely used than in a more environmentally deprived group of children, such as lower-class minority groups, and this might provide a test.

KENDLER: There are two assumptions that are implicit in what you said; namely that the neurological development is class-linked and that it is influenced by experience.

It is possible that experience furthers neurological development and that we might have a method for measuring such progress. However, showing a difference between social classes would not provide the evidence to link the behavior to underlying neurological functioning.

However, I should tell you something about the population. The first study was done in a suburban area with a rather high socioeconomic level, but the other studies were done in New York City and included a large group of children who were of a lower socioeconomic level.

We have, in our more recent work, been trying out crude measures of mental age in order to attempt to equate or measure some of these considerations. However we have not yet been very successful with the younger ages.

Now, to turn to the question about whether we can speculate about the nature of this thing, I am rather struck with the fact that two independent studies suggest that, at about six years of age, one half of the children are "mediating" or "conceptualizing." The fact that society seems to have picked this as the educable age, before we presented this kind of research, suggests that there is something in this.

I do not know yet how much of this will be controlled by purely experience variables. The idea of supplying the relevant verbalization was a beginning attempt to see if we could control the stimulus situation for these children: Will they necessarily mediate? The answer, although not clear, seems to be no.

Now that does not preclude the possibility that we could, nevertheless, by virtue of training facilitate the interaction of these two parallel processes, and we expect to do a lot more research to find the answers to some of the questions you have raised.

COFER (*New York University, New York, N. Y.*): One thing that bothers me somewhat about your experiment on the effect of verbalization is the verbal response you supply. As I understand it, it is *either* large or small, but not both, for the same subject.

Now, if I understand you correctly, the reversal shift of course involves a change in orientation from large to small. I wonder how you think such a response shift is accomplished.

KENDLER: First, consider the procedure. If, for example, large is going to be correct, we start out by asking the child; "Is it in the large one or the small one?" At the outset of learning the children respond by sometimes verbalizing large and sometimes small. After a while, they do however stop naming the incorrect stimulus and name only the correct one, for example: "It is the large one."

However the question you are asking, I think, is a fundamental one to which

we do not yet have an answer, namely, what is the basis of this mediation? Is it that they have learned a dimension, that is, they are attending now to the brightness; or is it that there is a verbal association between black and white or large and small in which the response to one automatically increases the probability that the other will occur. I am not sure yet what the process is. It certainly seems that it is a mediational one, but what the nature of this mediation is, we are not sure.

QUESTION: Concerning the discrepancy between your verbal measure and your behavioral measure, is it possible to conceive of each of these as a less-than-perfect measure of this mediating construct and, hence, the correlation between them would not be expected to be perfect. As I understand it, your discrimination procedure does not agree perfectly with the verbal measure.

KENDLER: You mean the tendency to choose reversal or not?

QUESTION: The discrimination procedure did not agree perfectly with the verbal identification.

KENDLER: We could not control the children's choices on the basis of verbal manipulation alone. Would that be it?

QUESTION: I could say that neither the verbal manipulation nor the reversal is a perfect measure of this mediating operation. Hence, since each one is not perfectly correlated with what you are trying to obtain, you would not expect the two measures to be perfectly correlated with each other.

KENDLER: If you are saying that one does not get results because an accurate measuring instrument is not available, I will of course accede to that, but we did get differences. We were able to demonstrate that the verbal mediation, if you combine both age groups, does exactly what it could be expected to do.

Does that answer your question? All of this is so new that I have not even been able to determine the significance of the differences between individual means. The over-all F test *is* significant. Relevant verbalization is better than no verbalization and irrelevant verbalization is worse than no verbalization.

This of course does not mean that we have a perfect measuring instrument, but it certainly demonstrates that it is effective in controlling or influencing results.

QUESTION: When did you decide that the child was a mediator?

KENDLER: Maybe I can relate this now in an extemporaneous way to the reversibility notion of J. Piaget. What we have been saying for a while now is that, if a child can reverse easily, that is, if he can do the opposite of what he did before more easily than he can do something else, then he is a mediator. Such mediation is an important step in the development of thinking.

Part III. Measurement of Thinking

SERIAL ANALYSIS OF THINKING

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As the other papers in this monograph demonstrate, there are many ways to study thinking. In large part, it seems that we are trying to understand what happens during those brief episodes of concentrated intellectual activity that occur when some kind of problem interrupts the routine activities of ordinary life. It is not altogether necessary, of course, to analyze such a problem-solving episode. There is much to be learned by treating it as an undifferentiated whole and using over-all measures of outcome, such as type of solution and time required, for comparison with antecedent variables. Attempts at analysis usually promise some additional information not obtainable from these conventional measures and should be evaluated in respect to this promise.

The most familiar analysis in the past was that by John Dewey (1910), who analyzed reflective thought into five steps, preparatory to explaining to schoolteachers how thought may be studied. Then Graham Wallas (1926) published a similar analysis reducing thought to four stages, of which "preparation" and "incubation" are perhaps the best known today. In later research on thought processes investigators have requested the thinkers to talk as they worked and then tried to classify the verbal output in categories of functional significance. Whereas the early writers seemed to assume that the real thought processes would be revealed by the introspections of an astute psychologist, current methodology views such research as attempts to demonstrate the consistency and utility of a coding system (Johnson, 1955).

Other analyses have been based on sequences of overt behavior. At first the experimenter simply watched the movements of his subjects and reported that he observed trial and error, insight, or perseveration. More recently the rules for identifying a response sequence have been written in advance, and the frequency of such a sequence compared with chance frequencies or with frequencies under other conditions. It is not common in experimental psychology today to attempt a complete description of problem-solving activities; rather the experiments are usually planned so that only those patterns of activity pertinent to a particular hypothesis are observed, and such patterns are not called thought processes because this research stems from a different tradition. However, in a broader sense the aim is the same, that is, to describe problem-solving activities in functional units that are larger than the single response and smaller than the whole problem-solving episode.

A sophisticated example of this approach is the study of concept attainment by Bruner *et al.* (1956) in which the subjects selected cards one after another in an attempt to discover the concept that the experimenter considered to be correct. From the mathematics of the case and the limitations of human memory, the investigators were able to describe certain ideal strategies, such as focussing and scanning, and then to observe how frequently such strategies were followed under different conditions and with what success. The appara-

tus used by John (1957) required his subjects to press buttons in sequence, and the records thus obtained permit description of the subjects' activities in several respects, such as the time at which the problem-solving activity shifts from a process of analysis to one of synthesis.

The experiments to be reported here also analyze problem-solving sequentially but follow the ideal of the older speculations and attempt to describe the whole problem-solving enterprise in terms of sequential thought processes. Nevertheless the early analyses attempted too much, identification of four or five separate processes, with an inadequate method, introspection. The present attempt is more modest and more objective. The most manageable number larger than one is two, thus the analysis is a two-part analysis. The problems are simple ones that are solved in less than a minute, and this brief episode is divided into two intervals by control of the exposure of the problem material during which two different intellectual activities are presumed to take place serially. A general description of thinking, outlined several years ago (Johnson, 1955), suggested that the first step, commonly called preparation, was the locus of many errors and was often overlooked by researchers, hence preparation was selected for special study and the remainder of the problem-solving episode was designated "solution." A simple model of cognitive dynamics attempts to describe the two processes preparation and solution, especially the dependence of the second on the first, and to make predictions that can be tested by the serial-exposure method.

The methodological problem of separating one activity from the next has not been discussed to any extent by psychologists, but two extreme possibilities can readily be imagined. We can imagine these two activities as completely different and completely separated in time and, if we knew where to draw the line, we could divide them clearly. Also, we can assume that the problem-solving episode is undifferentiated and that wherever a division is made the events in one group would be indistinguishable from those in the other. However everything we know about living organisms suggests that the most likely possibility is neither complete separation of the two processes (——————), nor no separation (————), but partial overlapping (————). Reading aloud, for example, includes a perceptual process and a vocalization process, and the first no doubt precedes the second, but the two overlap to the extent of the eye-voice span. Regressive eye movements when the material is difficult indicate even greater overlap. Preparation for problem-solving may likewise precede solution and continue after solution attempts have started. It is an empirical question whether, in any particular type of problem, the overlap is too great to permit temporal separation of the two activities. The problems of the following experiments were chosen to show in which cases separation of the two processes is possible and, where it is possible, to describe the preparation process and the variables that influence it. Some of the individual experiments will be described in more detail elsewhere.

Apparatus and Method

The serial-exposure box consists of two chambers separated by a partition and separately lighted. The side toward the subject includes a half-silvered mirror about 7 × 9 inches, and on the back side is a holder for 5 × 8 cards

positioned so that when the light in the left chamber is turned on the left half of the card is visible and when the light in the right chamber is turned on the right half of the card is visible. The problem material is separated in two parts and typed on the card as follows:

What number turned on	the letter
its side looks like	m

The answer to each problem is a letter from A to E or a number from 1 to 5, which is expressed by pushing one of five appropriately labeled buttons. A system of interval timers, relays, and clocks permits considerable flexibility of exposure conditions and timing. The experimenter can expose each side for a fixed interval of time or the subject can expose each as long as he wishes, and various combinations of these exposure conditions have been used in these experiments.

The main assumption of the method is that the thinker in his effort to solve the problem operates serially on the material that is presented serially. While the material on the left half of the card is exposed to view, the thinker has no problem to solve, but he is presumably getting ready for the material to be presented next and his activity is called preparation. His activity during the next period, which eventuates in selecting a solution and pressing a button, is called solution.

The subjects of the experiments were all college students, recruited in the usual way from elementary psychology classes.

Effects of Distraction

The first experiments explored the effects of distraction on preparation in order to see if anything of consequence takes place during this period. The problem is actually solved during the solution period and one would certainly expect that distraction during this period would interfere with solution but, if preparation is important to the solution, distraction during the preparation period would interfere also. To test this hypothesis 15 short problems were constructed, similar to those that appear on intelligence tests, and each was divided into two installments, the first giving the orientation to the problem, the second being a key word, letter, or number, as in the illustration above. The first or preparatory part of the problem was exposed for 5 sec., then the solution material was exposed until the subject indicated his solution by pressing a button. Distraction in the form of a loud doorbell under the exposure box extended for the duration of the preparation period, or for the duration of the solution period. Two experiments with about 100 subjects showed that distraction in either period increased the number of errors compared to control conditions. In one experiment preparation seemed to be more vulnerable to distraction than solution, but in another the direction of the difference was reversed. It was clear that preparation is worth serious study but that better control of exposure conditions was needed.

Controlled and Uncontrolled Exposure

The possibility of controlling both exposures was next explored. Thirty short problems of variegated verbal, numerical, and spatial content were given

to 5 groups of 15 subjects each, equated as to mean scores on a standard intelligence test. Each subject was allowed 12 sec. for each problem, but the 5 groups differed in the way this 12-sec. episode was divided between the two exposures. One group had 2 sec. for preparation and 10 for solution, another had 4 and 8, another 6 and 6, another 8 and 4, and finally 10 sec. for preparation and 2 for solution. The interval timers and relays exposed the right half of the problem immediately after the left half, with no blank between exposures and no overlap of exposures. Under these conditions there was a significant difference between the groups in number of errors. The 2/10 group made a mean of 13.7 errors, the 4/8 group 10.7, the 6/6 group 13.1, the 8/4 group 13.5, and the 10/2 group 18.3. Hence the optimal condition was 4 sec. for preparation and 8 for solution, and the worst were the two extreme conditions. Evidently, with these brief heterogeneous problems, the 12-sec. problem-solving episode is not entirely undifferentiated. A certain minimal time is necessary for each operation, perhaps about 3 sec., and extra time for one cannot be substituted for a shortage of the other.

Another way to assess the data of this experiment is to describe each of the 30 problems in respect to the relative difficulty of preparation. Many investigators have commented on the lack of standardization of problems used in problem-solving research and have asked for a method of comparing problems in some dimensions other than errors and time of solution. It is possible to scale these problems by dividing the number of errors made with short preparatory exposures (2/10 and 4/8) by the number of errors made under all exposure conditions. These indices range from 0.20 to 0.67, showing that some problems are much more vulnerable to short preparatory exposure than others or, alternatively, that some are more vulnerable to short solution exposure.

The latest method that we have developed for presenting such problems in the serial-exposure box is called uncontrolled or self-paced exposure. The subject has control of a centrally mounted double-throw switch that he uses when he is ready to switch the lights from the preparatory side to the solution side. He pushes a response button when he has selected a solution. Thus we have a record for each problem of time spent on the left side (preparation time), time spent on the right side (solution time), and the solution selected. When this method is used to scale problems in respect to relative difficulty of preparation, the times are converted to logarithms and the index is preparation time divided by total time. With 40 subjects and the same 30 heterogeneous problems these indices ranged from 0.32 to 0.70, indicating that the subjects spent much more time preparing for some problems than for others.

We now have two indices of relative difficulty of preparation for each of these 30 problems. If the two methods of scaling problems in this respect are dependable, the scale values should agree, and in fact the correlation between them is 0.68. Hence we can say that those problems that are failed most often when the preparatory exposure is short are generally the same problems on which other subjects spend the largest amount of their time in preparation. The uncontrolled or self-paced method of presenting the problems is simpler and more convenient than controlled-exposure, and it seems more natural in that it offers less interruption to the continuity of the problem-solving enter-

prise. After one demonstration the subjects flip the switch as automatically as they turn the pages of a book or look from one side of the page to the other.

The Question of Artificiality

At this point the question of artificiality was considered. It seemed possible that exposure of the problem material in two parts, serially, makes the problem a peculiar one, and that the method does not study genuine problem solving. This critical question was answered in three ways.

An intelligence test was used to match the groups in the above experiment by the controlled-exposure method, and scores on this test correlated 0.47 with number of problems solved. This correlation based on all 75 subjects is an underestimate for several reasons, but it does show that problem-solving performance under these conditions has something in common with the solution of the problems on a standard test of intelligence.

These problems were given to another group with both chambers of the serial-exposure box lighted so that the whole problem was visible for 12 sec. Under these more natural conditions of complete exposure, mean performance was only slightly better than under the optimal serial exposure, and the difficulty values for the 30 problems correlated 0.64 with difficulty values from controlled exposure and 0.72 with difficulty values from uncontrolled exposure.

When these problems were printed in the usual format without any separation and presented to psychologists for subjective estimates of relative difficulty of preparation, their mean estimates correlated reasonably well with comparable scale values obtained by the two objective methods: with controlled exposure 0.42, with uncontrolled exposure 0.56.

These three checks demonstrate, therefore, that the separation of the problem material into two parts for serial exposure does not distort the problem very much, that the indices of relative difficulty of preparation have something in common with psychologists' understanding of preparation, and that the self-paced method is generally the better of the two.

Induction and Deduction

Now that we have a method for studying preparation time that yields results of some stability and significance, we are in a position to investigate the determinants of preparation time. The verbal analogy seems to be a convenient problem for this purpose because the four terms of the analogy divide themselves naturally into two and two and because different types of analogies can be constructed and predictions about preparation times can be made in advance. In the case of analogies, preparation consists mostly of induction, that is, formulating the relation between the first pair of words, and solution consists mostly of deduction, that is, applying this relation to the second pair.

A block of 25 analogies was constructed with the first pair of words more difficult than the second pair, for example, *Visit* is to *Invade* as *Friend* is to *Enemy*. Another block of 25 was constructed with the second pair of words more difficult than the first pair, for example, *Lose* is to *Win* as *Liability* is to *Asset*. If the first pair of words is more difficult, the analogy problem may be

said to emphasize induction and, if the second pair is more difficult, it may be said to emphasize deduction. Hence we would predict that the index of relative difficulty of preparation will be higher for those analogies that emphasize induction.

The first pair of words was presented in the left side of the serial-exposure box and the first word of the second pair in the right side. The subject was instructed to think of the solution word, which always began with a letter from A to E, and to press the appropriate button. The two types of analogies were mixed in irregular order in a list of 50 problems and given under controlled exposure to four groups of 10 subjects each, matched as to mean score on the Terman Concept Mastery Test. The exposures were 2/10, 4/8, 6/6, and 8/4. The corresponding mean errors were 20.1, 22.0, 21.5, and 22.6, and the variance due to exposure conditions was not significant. Evidently a preparatory exposure of as little as 2 sec. will suffice for perception of two words, and the relation between them can be formulated during the solution period if necessary. The overlap between the two operations can be so large a portion of the whole that separation is not possible by this method. Correspondingly, the indices of relative difficulty of preparation, computed by taking errors under short preparatory exposures as a fraction of all errors, did not differ significantly between the two types of analogies.

When the same list of analogies was given to 20 subjects by the self-paced method of exposure, a better differentiation between the two types of analogies was obtained. Log mean preparation time was greater for the analogies emphasizing induction, and log mean solution time was greater for those emphasizing deduction. Likewise the time index of relative difficulty of preparation differentiated the two types of problems significantly. Thus we can conclude that in the case of analogy problems the method of controlled exposure is not adequate because the thinkers can remember such a small amount of preparatory material and work on it during the solution exposure, but that the self-paced method is adequate. When the thinkers are allowed to control the exposures, the mean times do reflect the relative difficulty of the two different logical operations. Any method that does not fractionate the problem-solving episode could not obtain such a differentiation because the two types of analogies were practically the same in respect to mean errors and total time.

Amount of Information

In problem solving and any other task in which one operation depends on a preceding one the amount of information presented is probably an important variable. In the heterogeneous problems of the intelligence-test type the number of words in the preparatory material was not controlled and in the analogy problems the number was always two. In order to study the influence of amount of information on time required for preparation, we need a task in which the number of items can be systematically varied. The type of problem most suitable for this purpose asks the subject to think of a solution that meets specifications stated in terms of discrete items, such as adjectives. This type is common in real life and is convenient in the laboratory because such problems can be constructed with varying numbers of specifications and be-

cause exposure of the specifications can be separated from exposure of the alternative solutions. A problem with four specifications is illustrated below:

gummed	1. map
descriptive	2. book
flat	3. label
readable	4. paper
	5. globe

The subject is instructed to study the material on the left side and to be prepared to find an object on the right that meets these specifications. The number of specifications was varied from 3 to 11 in order to get data on both sides of the span of immediate memory. Preparation for a problem of this type presumably consists of memorizing or organizing the specifications in some way and one may confidently expect that preparation time will increase with an increase in the number of specifications. The effect of this variable is something like what Bruner *et al.* called "cognitive strain." The number of alternative solutions was held constant at five, so if time spent on selection of solutions does not increase similarly, we may conclude that preparation is complete when solution commences, that preparation does not overlap solution.

Another variable was introduced in this experiment in order to make our description of problem-solving more complete. In problem-solving and many serial perceptual tasks, when the subject has difficulty making progress, he turns back if possible to a re-examination of the problem material, then searches again for a solution. Regressive eye movements in reading are a familiar indicator of difficulty and an analogous indicator in the self-paced serial-exposure situation would be switching the lights from the solution exposure back to the preparatory exposure. It would be expected that the number of such switchbacks would increase with the increase in number of specifications.

Four problems of each number of specifications from 3 to 11 were constructed, making 36 in all, and given to 20 subjects with switchback permitted and 20 without switchback. Those who were permitted to switchback had one switchback forced during the practice problems. The timing arrangements separated switchback time from solution time and added it to time of initial preparation.

The results turned out as expected. In the group not permitted to switch back median preparation time increased regularly from 5 sec. for problems of 3 specifications to 17 sec. for problems of 11 specifications. The group permitted to switch back showed the same regular increase but took somewhat more time in preparation for problems of more than 5 specifications—because of the increase in switchbacks upon reaching 5. In both groups solution time remained constant around 10 sec. in spite of variations in number of specifications on the preparation side. Thus for problems of 3 and 4 specifications solution time was longer than preparation time, while for problems of 6 or more specifications preparation time was longer than solution time. This appears to be reasonable in the light of previous discussion. When the preparatory material consists of only a few adjectives, one glance will suffice, and the thinker quickly switches to the other side to look for a solution, organizing these few specifications during the solution period if necessary. When there are 6 or 7 specifications, these cannot be grasped immediately but must be memorized

or processed in some way. This addition of specifications requires more time and is not always done thoroughly and switchbacks occur if permitted. In this experiment preparation and solution are clearly different processes in that their durations are differentially influenced by the independent variable, the number of specifications.

Returning briefly to the question of artificiality, we ran a group of 20 subjects on these problems with both sides of the problem exposed, but these subjects did not differ significantly from the serial-exposure subjects in respect to either time or errors. We surmised that they used a serial procedure even with complete exposure; at least whatever they do that is different from a serial procedure is not more efficient.

Formulation and Reformulation

The next experiment attempts to push the analysis beyond time comparisons toward a description of preparation. It is not possible, of course, to observe the preparation process, but it is possible on the basis of previous research to write a hypothetical description or model of cognitive dynamics and make predictions from it that can be tested by the serial-exposure technique. If the problem presents more material than can be easily grasped and retained during the search for a solution, the thinker memorizes or organizes this material in some way before attempting solution. If, in addition, the problem material is ambiguous or complicated, the thinker surveys the material selectively, ignoring some features and emphasizing others, and assembles those features that he has emphasized into a structure or formulation that he can retain and use in the search for a solution. Once the thinker has achieved a formulation of the problem, he prepares to search for solutions of the type that fit this formulation, hence it is possible to predict from formulations to solutions. It follows also that, if the thinker does not find the type of solution he is looking for, he will reformulate the problem and prepare for another type of solution. If the problem is an abstract one, like a concept-formation problem, reformulation changes the cognitive side of the set, for example, from observing colors to observing shapes, while the motor side of the set remains constant.

To test this simple model of cognitive dynamics and validate this mediating cognitive construct that we call formulation, we need problems that permit two distinct formulations and two correspondingly distinct solutions, and an independent variable that slants formulation toward one or the other. Figure concepts were used because it is easy to draw many sets of figures that can be conceptualized in terms of their shape, many others that can be conceptualized in terms of their markings or texture, and many ambiguous ones that can be conceptualized either way. Ten figures were drawn on the left half of a card and ten others on the right. With such a card in the serial-exposure box the instructions are to observe what the figures on the left have in common, then to switch to the right and find another illustration of this concept.

Ambiguous problems were constructed for the first group of 24 subjects. That is, the preparatory material consisted of ten figures that were similar in respect to both shape and texture and could therefore be formulated either way, and the solution figures included one that fit the shape formulation and

one that fit the texture formulation. When the problems are ambiguous in this sense, selection of the shape solution indicates that the problem was formulated in terms of shape similarities and selection of the texture solution indicated that it was formulated in terms of texture.

The independent variable intended to slant formulation toward either shape or texture was pretest experience. Rees and Israel (1935) and Luchins (1942) have demonstrated that a set for a certain type of solution can be induced by previous experience with a series of problems for which such solutions are obvious. The hypothesis is that the pretest experience slants the formulation toward a general class of similarities, for example, shape, then, when the preparatory material of a test card is presented, the subject will be set to observe shape similarities. If he finds a similarity of shape, for example, triangles, he adopts a more specific set for this subclass, switches to the solution side, and looks for a triangle, ignoring many other things along the way. Half the subjects had 8 easy pretest shape problems and the other half had 8 easy texture problems. Then all subjects had the same three ambiguous test problems, and the number of set solutions was 89 per cent, far above chance levels. Those who had had the pretest shape series selected shape solutions and therefore must have formulated the test problems in terms of shape, while those who had had the pretest texture series selected texture solutions and therefore must have formulated these same problems in terms of texture similarities. Thus we have demonstrated the role of previous experience in formulation and we have a technique of manipulating formulation for the next experiment.

Now suppose the subject, having formulated a problem, looks confidently for a solution that fits his formulation, but does not find one. Everyone is familiar with this sequence of events. Our model of cognitive dynamics would predict that, when the amount of preparatory material exceeds the memory span, the disappointed thinker will switch back to the preparatory material and reformulate the problem, then return and look for a solution to fit this new formulation. In the case of the concept-formation problem this reformulation would consist of trying to find a different dimension of similarity among the 10 figures. The switchback is an observable indicator of reformulation, so the hypothesis is simply that, when the test problems do not have a solution that fits the formulation developed during the pretest series, the subjects will switch back, then return and find a correct solution.

The pretest series were the same as before, but the 12 subjects who had pretest shape problems now had test problems with a texture solution only, and the 12 who had pretest texture problems had test problems with a shape solution only. The results were as expected. Every subject switched back to the preparatory material at least once, and the total number of switchbacks was 43, to be compared with 9 for a control group. The percentage of correct solutions was 83, to be compared with 57 for a group given the same sequence of problems but not allowed to switch back. The data on preparation time and solution time are also congruent with this analysis. The evidence is clear, then, that we can describe and experimentally manipulate not only formulation and solution of these concept problems but also failure, reformulation, and solution on the second try.

Limitations

These studies have been concerned with the solution of short problems finished, for the most part, in less than one half of one minute. Obviously if the times were much longer, the activities could shift back and forth, and serial analysis might be unprofitable. Also, these problems have been solved by selection from a display of solutions. In the experiments planned next the subject will produce his own solutions.

Conclusions

These assembled studies justify the following conclusions:

It is worthwhile to analyze thinking serially, by serial-exposure of portions of the problem, and the data obtained are more descriptive than the conventional data on total time and errors.

The overlap between one thought process and the next is smaller when the thinker himself controls the presentation of the material.

Efficiency of problem-solving under serial-exposure differs little, if at all, from efficiency under complete exposure.

It is possible by this serial-exposure technique to identify, in the solution of a variety of types of problems, a preparation process that precedes the actual selection of alternative solutions.

Preparation, as thus identified, differs from solution in respect to the functional relation between the time spent on each and certain independent variables, such as type of problem and amount of preparatory material.

There is a large class of problems, preparation for which consists of formulating the problem in terms of a dimension of similarity among the items and, when there are alternative dimensions of similarity, this formulation is influenced by immediately preceding experience with similar problems.

The difficulty in solving a problem may be quantitative when there is too much preparatory material to remember, and in such cases the thinker is likely to switch back to the preparatory material for more adequate preparation and to be more successful on the next try.

The difficulty may be qualitative when the formulation does not fit the available solutions, and in such cases also the thinker will switch back to the preparatory material to reformulate the problem and is likely to be more successful on the next try.

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RELATIONS BETWEEN MOTIVATIONAL CONDITIONS AND THINKING*

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The subject of this paper has a long history of efforts to explain the organization of "consciousness," or the relation between the environment, both external and internal, and the ongoing behavior of the individual (Vinacke, 1952). This history has to do with the psychology of attention, with psychoanalysis and its emphasis upon the unconscious, ego mechanisms, and primary and secondary processes, with the dramatic work of the Würzburg School on set and attitude, and with the history of the law of effect (Postman, 1947).

Despite these historic issues, it has only been during the past couple of decades that the experimental laboratory has started to come to grips with motivational variables. Students of animal behavior, to be sure, have not ignored motivation, as witness studies of incentives and deprivation. However, aside from some investigations of reward and punishment, the recall of pleasant and unpleasant experiences, and a scattering of other studies, experimental psychology has largely ignored motivation until recently. In fact, human subjects have been treated as if they were very much alike, motivationally speaking.

In an earlier work on thinking (Vinacke, 1952), the importance of motivational factors was recognized, but scarcely developed. This casual treatment reflected the status of research at that time. Since then the rapid growth of interest in motivation has opened numerous doors previously screened, if not closed. A large share of the credit must go to the steadily mounting influence of clinical psychology in which motivation cannot be ignored. Part of the credit also belongs to the fertile influence of people like the late Kurt Lewin and David McClelland. Whatever the reasons, motivation has moved rapidly to the center of the psychological stage, accompanied by a great deal of theorizing which, in turn, is leading to important research.

The very large body of accumulated material provides a vantage point in discussing relationships between motivation and thinking, and hence in achieving a possible clarification of many baffling problems of human complexity. We may thus begin to feel less embarrassed by the necessity of invoking "individual differences" when systematic principle fails.

Before proceeding further, however, it will be useful to discuss briefly the terms thinking and motivation.

Thinking

Textbooks amply document the fact that thinking is a name given to a variety of behavior, rather than to some unitary process. This broad category includes problem solving, decision making (whether immediate or delayed), judgment, imagination, and creative processes. For my purpose, two common denominators of these activities may be identified.

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In the first place, thinking is the utilization of past experience, in contrast either to the acquisition of experience (learning) or to the immediate relations between stimulus and response (perception). The term experience connotes the retention in some form or degree of what has previously happened to a person, and thus signifies that symbolic processes are involved: images or words or other processes that, following the Würzburg precedent, may be called attitudes.

In terms of the ongoing behavior of the individual, what we call thinking depends merely upon the kind of emphasis we choose to give to cognitive behavior, almost all of which involves prior experience. If a subject's immediate relation to the environment is considered, the focus is upon perceptual processes. If the interest lies in the relatively enduring changes produced by specified stimulus conditions, the concern is with learning. Emphasis upon thinking means to consider how prior learning enters into a present situation. The more it is necessary to refer to variables not represented by the immediately attendant properties of the situation, the more likely it is that we are dealing with the phenomena known as thinking.

In the adult, at least, perception, learning, and thinking can be distinguished only as a matter of convenience, although experiments may be carefully devised to emphasize one aspect rather than another. The intimate and continuing relation of perception to learning and thinking has been spelled out by Woodworth (1958). The overlapping of thinking and learning is represented by the tendency to speak of "complex learning," a phrase, I think, intended to stress the fact that in certain kinds of tasks, that is, those associated with problem solving, an individual is changed by his behavior—that is, learns—just as much as he utilizes past experience: that is, thinks. I shall, however, make the latter feature primary.

A second essential feature of thinking is that it cannot be separated from the motivational state of the individual. All thinking is goal-relating or wish-fulfilling behavior, or compounded of both. In either case, the central reference to motivation is inescapable. We may well adopt Murphy's (1947) formulation of thinking as ranging between autistic and realistic poles. At the autistic extreme, internal motivational states determine cognitive processes with only a minimal influence of environmental conditions, as in free association, fantasy, and dreams. At the other extreme, thinking is strongly influenced by external environmental conditions: immediately perceived goals and attendant circumstances, as seen in problem situations. A continuum lies between these extremes, neither of which is approached in a pure form except under special arrangements, such as those that may approximately be established by an experimenter or clinician. Thus, although imaginative processes may be distinguished from realistic ones, both are inextricably bound up with motivational variables.

Putting these two criteria together, then, thinking may be regarded as the utilization of past experience in response to motivational states.

Motivation

Like thinking, the term motivation covers many aspects of behavior. It will here refer to the inferred processes that instigate, regulate, and adjust be-

havior. Since all these processes enter into every act of the individual, the analysis of motivation is not a matter of deciding which process is most important, but of differentiating components of the motivation system and their interrelations. In recent years a number of more sophisticated theories have superseded the traditional instinct view, but space precludes discussion of these exceedingly interesting and important issues. Instead, I shall present a schema based upon a *drive-modification* theory, which has been treated in some detail elsewhere (Vinacke, 1960). In doing so, I shall be forced to ignore the role of learning in shaping the motivation system and shall simply assume that there may be recognized in the adult human being several levels at which motivational variables can be formulated.

Arousal. At the fundamental level, the inference can be made that the organism varies in degree of activity. This is sometimes phrased as an energy or need concept and contains the supposition that variations in biological processes are accompanied by changes from a low or quiescent state to a high or aroused state. In psychology the arousal level is usually treated simply as a postulate attendant upon the fact that the individual is alive.

Instigation. Arousal leads to various kinds of activity. That is, energy is mobilized or utilized in different ways, depending upon which internal processes are predominant. These predominances are called drives in their primitive, unmodified form, and motives in their derived, modified form. Without considering here the many controversial issues associated with conceptualization at this level, I shall merely assume that there are many energy-utilizing components of motive systems, including visceral, sensory, emotional, muscular, and neural; that instigation depends upon the organization of these components; and that the names we give such organizations as hunger, cognition, aesthetic, athletic, and fear correspond to the predominance of certain components. Furthermore, I shall assume that learning may result in the establishment of predominances that cannot be traced directly to any of the just-mentioned components. This kind of predominance may be called psychogenic instigation (Murray, 1938). Examples include achievement and affiliation.

It is plain that, according to this view, the organism is always in a state of instigation, albeit more or less aroused. Changes in predominance (or kind of motive) come about from internal shifts in organization (intrinsic arousal) or from external stimulation that affects the mobilization of energy by various components of the system (extrinsic arousal).

Regulation. Behavior that results from or accompanies instigation, whether internally or externally aroused, varies in such factors as intensity, quality, and content. It consists of certain kinds and degrees of response, to the exclusion of others. The mechanisms inferred to be responsible for regulating the general course of instigation may be called attitudes.

Adjustment. Finally, behavior is also highly specific, since particular acts occur as a function of specific conditions. In addition to the regulation of motives, therefore, there is a continual process of specific focusing, whereby the individual adapts to particular internal and external conditions. The inferred mechanisms may be called sets.

Questions concerning the role of motivation can thus be cast at any of these four levels. What applies at one level does not necessarily apply at another

level. For example, the investigation of set phenomena may tell us nothing about instigation but only about minor variations in response when environmental conditions are varied under some given state of instigation.

Experimental psychology, indeed, has typically manifested characteristics that have worked to keep the analysis of motivational variables at the adjustment level. In the first place, subjects have been treated as if the only differences among them resulted from variations in the stimulus properties of the situation. Adaptation of subjects is certainly a function of specific and temporary sets, but it is unknown to what extent these may also be indexed to regulation and instigation properties. In the second place, the situation itself is almost invariably defined by the experimenter, so that whatever regulative or instigative process may operate in the subject is either ignored or assumed to be what the experimenter thinks it ought to be. As we shall see, recent developments are demanding drastic modifications of both these modes of treatment.

Approaches to Motivation

As psychology advances toward the explicit study of motivational variables, three major sorts of experimental manipulation are apparent.

Assessment of antecedent conditions. One technique is to determine the motivational properties that the subject brings into the experimental situation, or his intrinsic motivation.

Pretesting may be undertaken either to establish degrees of intensity within a given motivational category, for example, to identify persons high and low in achievement; or, to assign subjects to different instigation categories, such as high achievement and high affiliation. Tests such as the Taylor Manifest Anxiety Scale, the French Test of Insight, and the Thematic Apperception Test have been utilized for these purposes.

It is clear that these instruments are designed to get at the instigation level, without distinguishing the arousal level. Of course, pretesting can be and has been used for assessment of regulative properties also.

Manipulation of task conditions. In a second approach, the experimenter tries to influence the motivational system through the environmental situation that confronts his subjects. Ways in which this may be accomplished include (1) varying the task itself, for example, by making it easy or difficult, simple or complex; and (2) varying the subject's relation to the task, for example, by arranging a program of reward and punishment or by introducing threat or ambiguity; and (3) varying the subject's relation to the experimenter, for example, by creating a warm or rejecting atmosphere.

This kind of technique is most likely to have significance chiefly for the regulation and adjustment levels, since the conditions imposed upon the subject bear upon the manifestation of instigation in particular goal situations.

Arousal or instigation. Finally, an experimenter may deliberately attempt to influence the instigation level by stirring up the subject. Investigators have often employed the word arousal in this connection without making the distinction followed in this paper. Indeed, in any given situation arousal and instigation are synonymous, for it is only when diverse forms of instigation are compared that the quantitative concept of arousal appears.

This third technique has two versions. In external arousal, or experimental induction, the experimenter creates conditions expected to engage some specified motive. This aim may be accomplished by instructions, as when achievement instigation is presumably evoked by telling subjects that the task is a difficult intelligence test, or by direct stimulation, as when college boys are shown pictures of nude females. The other version may be called internal arousal or deprivation as when the experimenter, emulating studies of animal drive, withholds for various periods some kind of goal-object, such as food, water, or social stimulation. Only limited use of this technique has so far been made with human subjects, but ingenious investigators are pointing the way (Gewirtz and Baer, 1958), and it is conceivable that almost any kind of instigation might be manipulated in this fashion. Research has not yet clarified the extent to which deprivation and experimental induction have similar or different effects.

There is of course no reason why more than one of these techniques should not simultaneously be employed. Just this kind of joint variation is in fact being widely undertaken, as we shall see, especially in relation to the problem of relevance.

We may now turn to the kinds of relation between motivation and thinking that emerge in research. I have found more than 250 pertinent studies, but a large proportion of them properly belong in the fields of perception and learning, and therefore provide only indirect perspective on thinking.

Arousal and Instigation

As previously stated, arousal is a general term that refers to the mobilization of energy, or the activity level of the organism, defined in relation to some kind of instigation, such as hunger. It is conceived as lying along a continuum from a low or minimal degree to very high degrees. It may be presumed that instigation of any kind varies in this manner and, theoretically at least, equivalent degrees of arousal should be measurable for different motives. However, no one yet knows how to arrive at this comparability. The problem is complicated by the probability that individuals differ widely in degree of arousal under the same experimental conditions and also by the likelihood that experience with a situation modifies the degree of arousal in the same person.

Although the concept of arousal or activation has received considerable attention in recent years (Duffy, 1957; Lindsley, 1951; Malmo, 1959; Bindra, 1959), there is little agreement on either the physiological or psychological mechanisms involved. Nevertheless, although arousal is evidently not a unitary state, as shown by the fact that physiological indices are not highly intercorrelated (Bindra, 1959, pages 215ff), some measures at least are related to cognitive behavior. Thus Vogel *et al.* (1959) have demonstrated that physiological reactivity, as measured by autonomic changes, is associated with performance on symbol counting and scrambled-word tasks.

Research bearing upon the general principle of arousal falls into two major phases.

Tension. In the first period investigators sought to relate increases in muscular tension to cognitive efficiency. In most of these studies (Bills, 1927;

Bills and Stauffacher, 1937; Courts, 1939; Freeman, 1938; Shaw, 1956) performance in simple tasks has been found to improve when the subject grips a dynamometer. That is, the subject worked harder, faster, and typically more accurately with increased tension. In some experiments, however, this relation was not found, and there is an indication that it may not hold for complex tasks like problem solving (Bills and Stauffacher, 1937; Block, 1936; Freeman, 1938; Zartman and Cason, 1934).

Particular instigation. More recently the arousal of specified modes of instigation has been investigated, stemming especially from the work of McClelland and his associates (1953). Although achievement instigation has been a central interest, other motives have also been studied: affiliation (Shipley and Veroff, 1952; Atkinson *et al.*, 1954), power (Veroff, 1957), sex (Clark, 1952; Clark and Sensibar, 1955), aggression (Feshbach, 1955), cognition (Cohen *et al.*, 1955), and Hunger (Sanford, 1936, 1937; Wispé, 1954; Wispé and Drambarean, 1953).

In these studies it has been demonstrated that imaginative processes, as measured in stories told in response to selected thematic apperception pictures or in free association, reflect the motive aroused. For example, instructions intended to instigate achievement increase the incidence of themes rated as having achievement content. This effect, however, is far from identical in all modes of instigation. Thus sex themes may not appear directly in the stories, but rather in symbolic form (Clark and Sensibar, 1955). (A similar result occurred in an experiment by Mussen and Scodel, 1955.) There is evidently a complex interaction between instigation and regulative processes, such as repression, that under certain circumstances changes the manner in which instigation will be expressed.

Whether behavior associated with given degrees of intrinsic instigation is similar to that brought about by experimental induction or deprivation remains uncertain. McClelland *et al.* (1953) point out that high achievement scores may be obtained from some persons under neutral conditions, from others only under appropriate instructional orientation, and also simply from presenting pictures with strong achievement-related cues. It is implied that all these scores are valid indicators of achievement arousal. It may be, however, that different levels of motivation are tapped, with corresponding differences in the interpretation of accompanying cognitive behavior. Studies by French (1955), Lazarus *et al.* (1957), and Vogel *et al.* (1958) are among those suggesting that experimental induction has quite different consequences for persons high in relevant intrinsic motivation and for those low in this respect. Much more research on this point is needed.

Only very limited data are available on the relation between instigation and realistic thinking. Most research has been concerned with achievement motivation. Scanty evidence exists for the association between high achievement and better performance in problem solving (McClelland *et al.*, 1953), even though persons so described may be more productive verbally. As suggested by Vogel *et al.* (1958) and others, the effect of instigation depends upon regulative processes evoked by the situation. This point is nicely illustrated by Birney's (1958) replication of Lowell's (1952) experiment. Lowell reported

that subjects high in achievement solved more arithmetic problems and displayed more rapid learning on a scrambled-word test than did subjects low in achievement. Birney failed to obtain this difference when the subject was a faculty member, but confirmed Lowell when a student was used. Thus, like the results with sex instigation, even without the introduction of such a factor as stress, the relation between instigation and thinking is partly a function of processes at the regulative and adjustive levels.

Considerable research has been done with manifest anxiety as a possible index to arousal. It has been shown that no simple effects are correlated with this variable whether it is assessed by pretest or experimentally induced; for, again, it depends upon task conditions (Child, 1954).

For example, in the case of rote learning, Taylor and Chapman (1955) reported better performance for subjects high in anxiety, whereas in other experiments there was no difference between high and low groups (Hughes *et al.*, 1954). A survey of about 40 experiments suggests that subjects low in manifest anxiety are likely to behave quite efficiently regardless of experimental conditions, whereas persons high in anxiety are dependent upon task conditions. These individuals tend to be adversely affected, on the average, by stress or complex or difficult tasks (Sarason, 1956, 1960*a* and *b*).

In the handful of experiments where the task could be called problem solving or complex learning (Beier, 1951; Fattu *et al.*, 1954; Gaier, 1952; Romanow, 1958; S. B. Sarason *et al.*, 1952; Truax and Martin, 1957), results indicate that persons high in anxiety perform less efficiently than persons low in anxiety. Once more, therefore, the effect of task variables appears, showing the importance of regulative and adjustive factors. Indeed, manifest anxiety may not be a measure of general arousal at all but rather an indicator of some general attitude evoked by certain kinds of situation. Evidence for this hypothesis comes from studies in which a low negative correlation is consistently reported between anxiety and achievement scores (Kausler and Trapp, 1958; Raphelson, 1957; Raphelson and Moulton, 1958). Furthermore, in one experiment (Berry and Martin, 1957) anxiety showed no relation to skin conductance, generally regarded as an index to arousal; in another experiment (Raphelson, 1957) this relation was found for one criterion of anxiety but not for another. Raphelson indeed believes that anxiety is specific to the situation. If this is the case, then anxiety, as it has been measured, reflects regulative (or adjustive) rather than instigative variables.

From this all-too-cursory survey of the arousal problem, it is clear that no simple relation exists between increased instigation and thinking. Imaginative processes reflect the degree and kind of instigation, but regulative factors may greatly modify its expression. In the case of complex tasks such as problem solving, research is still too scanty to permit the drawing of conclusions at the arousal level. Certainly the degree and kind of instigation is bound up with task conditions, but whether this can be generalized in terms of arousal is not known.

The Optimum Principle

A logically succeeding question pertains to variations in behavior over a wide range of arousal. It has been several times proposed that this relation is

curvilinear (Bindra, 1959; Iverson and Reuder, 1956; Malmo, 1959; Murphy, 1956; Woodworth, 1958); that is, as arousal increases, there is a change in performance (a decrease for some measures, an increase for others) to some point of intermediate arousal, and then a decrease or increase beyond that point. The description of intermediate arousal as optimal means that performance at this degree is expected to be superior to either high or low arousal. However, in view of discrepant results, this supposition remains hypothetical.

Evidence for the optimum principle in simple tasks is generally favorable. For example, despite some disagreement (Block, 1936; Zartman and Cason, 1934), the tension studies previously mentioned showed that moderate tension has a more facilitating effect upon tasks such as rote learning (Courts, 1939; Stauffacher, 1937) and perception span for digits (Shaw, 1956) than either slight or great tension. Similar results have been reported in experiments relating various degrees of food (and thirst) deprivation to perceptual processes. Levine *et al.* (1942) found that food responses to achromatic stimuli increased chiefly after six-hour periods of deprivation rather than after longer or shorter periods. Failure to obtain these results with chromatic stimuli led them to suggest that the more realistic character of the colored stimuli tended to make them dominate autistic tendencies, a point consistent with the point of view of this paper. Wispé and Drambarean (1953) found that recognition thresholds for need-related words decreased with degree of deprivation, especially with moderate deprivation. An experiment by Postman and Crutchfield (1952) showed that set may markedly influence the expression of instigation. Thus, merely because they are hungry, people do not necessarily think about food. Environmental conditions affect the relation between instigation and behavior.

Evidence from research with imaginative processes also seems, for the most part, to be favorable to the optimum principle. In Sanford's early experiments (1936; 1937) the incidence of food responses in word association, picture interpretation, completion of drawings, and other tasks increased, especially with moderate deprivation. Within a more restricted range of deprivation similar results occurred for some, at least, of the TAT measures employed by Atkinson and McClelland (1948). Wispé (1954) found that the incidence of food responses in free association changed markedly at 10 hours of deprivation, in comparison with no deprivation and 24 hours of deprivation. This change was especially great for object words, which increased, and instrumental words, which decreased, with moderate deprivation. Clark and Sensibar (1955) found less sexual symbolism in the stories of subjects rated as moderate in manifest sex. The data of Cohen *et al.* (1955) indicate that persons moderate in "cognition" motivation differ from groups high or low in this measure. In their studies of achievement motivation, McClelland and his associates (1953) discovered that subjects falling towards the middle of the continuum produced fewer words in their stories but gave more Rorschach responses and had slower recognition time for achievement-related words.*

Realistic thinking, also, has sometimes been interpreted according to an

* There is not space here to discuss the proposed explanation and research prompted by these facts (Clark, *et al.*, 1956; Atkinson, 1957). Curvilinear formulations, however, characterize this later work also.

optimum principle. Probably the classic instance is Birch's (1945) study of insightful problem-solving by chimpanzees. The group moderately deprived of food solved problems more rapidly and produced a larger proportion of insightful solutions than either the nondeprived or the highly deprived subjects. In so far as I know, this study has never been replicated with human subjects.

A curvilinear relation between manifest anxiety (Taylor Scale) and maze learning has been reported by Matarazzo *et al.* (1955); moderate anxiety was associated with better time, but the relation to trials was linear. This indicates the necessity to differentiate qualitative and quantitative aspects of performance. Anxiety was also employed as a variable by Romanow (1958). In a concept-forming task, persons moderate in anxiety made fewer errors on hard concepts but more errors on moderately difficult and easy concepts; there was a similar association with moderate ego involvement.

Finally, it should be noted that Osgood and Walker (1959) found marked differences between suicide notes presumably written under conditions of high arousal and ordinary letters supposedly composed, on the average, under more moderate arousal. The suicide notes were characterized especially by stereotypy, indicating a decline in quality of language usage.

Thus there is a fair amount of evidence in support of the optimum principle. However, we are far from knowing either at what level of the motivation system to pitch interpretation or which qualitative or quantitative variables show an increase or decrease at middle ranges of intensity. Research with a variety of tasks and measures, comparing variations in both intrinsic and extrinsic arousal, is urgently needed. In any case, it is quite evident that "middle" as well as "high" and "low" groups deserve careful investigation.

In certain experiments, middle degrees of success and failure, stress, or ego involvement, have been included. These task conditions appear at this juncture to affect the motivation system more at the regulation or adjustment levels than at the arousal or instigation levels. In those studies that can be said to involve thinking rather than such factors as perception, rote learning, and recall, (Ainsworth, 1958; Cowen, 1952; Katchmar *et al.*, 1958; Yacorzynski, 1942), no optimum effect appears. This result further reinforces the significance of task conditions, especially in that the role of motivational variables may be quite different in complex compared to simple tasks. The observation that optimum arousal changes with experience—for example practice, familiarity, and success—in simple tasks (Bindra, 1959) for example, has, in so far as I know, not been tested in problem-solving or concept-forming situations, nor indeed in imaginative contexts.

Task Conditions

In contrast to the distinctly limited research that bears upon the arousal and instigation levels of motivation, there is a vast body of material concerned with the regulation and adjustment levels.

Within the short scope of this paper, it is possible to touch only upon a few points. I must therefore exclude most of the research on reward and punishment, variations in incentives and meaningfulness, and much more. All of this work shows that variations in the environmental conditions to which the

individual is exposed affect the intensity, direction, content, and specific form of behavior.

It is safe to say that, whenever a criterion of adequacy, such as the correct solution to a problem, writing a story, or attainment of a concept, is established as a means to assess performance, there is always implied some degree of organization in behavior. That is, something is inferred to take place that changes behavior from a nongoal-oriented (or a different-goal-oriented) character through a goal-orienting to a goal-attaining pattern. These inferred processes may be called attitudes and sets, or mechanisms that serve to control the course and outcome of instigation. Maier's (1940) well-known hypothesis of "direction" in problem solving, as well as concepts such as "centering" and "recentering" (Duncker, 1945; Wertheimer, 1959) imply at least adjustment processes.

Nevertheless, regulation must be differentiated from adjustment, for regulation signifies properties of organization that may be inferred to exist in evokable form prior to observed performance; these properties are considered to be relatively enduring and predictable general mechanisms that have been acquired to serve as vehicles for instigation, whether well or poorly. On the other hand, processes at the adjustment level account for specific acts or sequences of acts induced by a particular environmental situation.

This distinction is perhaps especially important in problem solving, where what the individual brings into the situation—preferences and values, self-defending and self-enhancing mechanisms, modes of attack—must be geared to circumstances such as available resources, perceived characteristics of the goal, and judgments of success or failure.

The influence of situational factors on thinking can be illustrated by experiments that have sought to manipulate stress conditions. Lazarus *et al.* (1952) point out that stress is not a general condition but depends upon the interaction of the motivation system and the situation. They suggest that stress occurs "when a particular situation threatens the attainment of some goal." Thus stress is a function of a person's definition of the goal and his relation to it (see also, Lazarus *et al.*, 1957).

One widely employed technique is to create conditions where subjects are exposed to success and/or failure, often by providing false information such as prearranged norms. The results, for once, are simply stated: when subjects are inferred to experience success their performance is facilitated and, when they are inferred to experience failure, their performance is adversely affected. In 28 studies surveyed, no exception was found with respect to success. With regard to failure, however, a few studies report a favorable effect. In these cases, either simple tasks have been used, such as rote learning or recall, or favorable effects occurred only for nonanxious subjects or with task-orientation instructions. The excellent experiment by Lantz (1945) illustrates the point. Lantz found that school-age children are adversely affected by failure on Stanford-Binet items that require reasoning but not on items demanding rote memory.

It is rather surprising that success-failure conditions have seldom been studied in problem-solving situations in view of results like those of Lantz. If behavior in complex tasks is especially sensitive to success and failure, then

we ought to give them intensive study. Perhaps a large amount of otherwise unexplained variation in performance is a function of unmeasured expectation of or reaction to success and failure. It is unlikely that the mere definition of task conditions as neutral rules out important experiences of these kinds.

It is often difficult, in fact, to determine the degree to which intended stress conditions pose threat to the subject. Techniques have varied widely, including badgering by an experimenter or stooge, ego-involving instructions, devices to induce anxiety, use of strong electric shock, and severe frustration. Scrutiny of about 73 studies reveals that what was defined as stress brought about, for the average subject, a favorable effect in only 25 per cent of the experiments, no effect in 14 per cent, and an adverse effect in 63 per cent. However facilitation occurred more often in simple than in complex tasks, especially for individuals low in measured anxiety. Once again, therefore, we see possible differences for tasks especially interesting to students of thinking compared to those assigned to learning and perception. Complex tasks may, of course, differ from simple tasks merely in optimum arousal as stress increases, but this is not known.

Qualitative effects are often mentioned in the few studies that introduce stress into more complex tasks where, probably, qualitative aspects of behavior are more likely to be noticed whether or not they are more important. These observations include a loss in abstract behavior (Beier, 1951), increased rigidity (Ainsworth, 1958; Cowen, 1952), increased variability (Lazarus and Eriksen, 1952), reduction in rational inference (Patrick, 1934), and reckless and premature guessing (Postman and Bruner, 1948). Stress appears to upset the subject's relation to the goal in complex tasks. It is certainly not clear, however, whether this effect results from the evocation of interfering responses (Child and Waterhouse, 1952; Sarason, 1960*a*), distortion of the goal or other features of the situation (Smock, 1955), disturbance of organization within the motivation system, or some other factor.

At the present time, it would be unwise to interpret stress variations in terms of the arousal continuum previously mentioned. Evidently there will first have to be much more research with many kinds of task under a wide range of stress increments.

The main point of this section has been to point out the influence of situational factors on thinking. Space does not permit the citation of other evidence, such as the interaction of stress with ego-involving instructions, anxiety, and other such factors. In general all this research clearly shows that cognitive processes are determined not only by intrinsic motivational properties but also by adjustive processes evoked by environmental conditions. However, neither set of conditions is independent of the other, a conclusion recognized in the concept of relevance to which I shall now proceed.

Relevance

The discussion thus far has increasingly revealed that distinctions among and relations between the various levels of motivation are of the utmost importance in accounting for thinking. Following the lead of Vogel *et al.* (1959), I shall call the gearing together of intrinsic properties of the motivation system

in relation to the environmental situation the problem of relevance. Similar intention can be seen in work by French (1955), Atkinson and Reitman (1956), and others. It follows also from my own earlier treatment of thinking (Vinacke, 1952).

Two major approaches to this problem are evident.

The first is represented by the persistent efforts of George Klein (1954, 1958) to explicate variables of cognitive style or general attitudes that govern behavior in related environmental situations. Klein attributes controlling, directing, detouring, and modifying functions to cognitive styles that may "deploy attention" with reference to the goal situation through scanning, simplification, and other methods, or they may affect the "anticipation of alternatives" through determining the flexibility or constriction of control. Subjects classified as constricted or flexible on the basis of a color-word interference test were compared when they were thirsty and sated. Under both these conditions the flexible control subjects overestimated sizes while the constricted control subjects underestimated. If no distinction had been made on the basis of attitudes, there would have been no evidence of distortion. In a tachistoscopic task, furthermore, there were no differences under the stated condition, but when they were thirsty the flexible subjects varied their fixations widely and proved to be more accurate than the constricted group. Loomis and Moskowitz (1958) suggest that flexibles are more likely to integrate competing, overlapping, and contradictory elements, while constricters tend to keep ambiguities apart.

Another kind of attitude appears in a transfer-retroaction study by Miles (1958). Subjects high and low in achievement imagery were classified as either analyzers or nonanalyzers from the character of their approach to the Wechsler Block Design Test. On a pursuit-meter task, the relation of attitude to motive emerged strikingly. For example, high-achievement analyzers were superior to low-achievement analyzers during early stages of original learning, after which the latter performed better than the former. In the relearning trial, high-achievement nonanalyzers showed the greatest loss and low-achievement analyzers the least.

Still another attitude is called failure-avoidance by Schroder and Hunt (1957). In problem solving, this attitude proved to be associated with setting unduly high goals, employing few alternative solutions and, in general, acting in an unrealistic manner to defend the subject from recognizing his own inadequacy (Scott, 1956).

Other experiments also cast light upon the subject's relation to goals. Masculinity as measured, for instance, by the Terman-Miles test correlates with problem-solving efficiency within each sex (Milton, 1957). Attitude toward problem solving itself is associated with better performance (Carey, 1958), a relation stronger for males than females.

My own research with triads playing a simple game adds another dimension (Vinacke, 1959). The two sexes interpret this situation quite differently. Males apparently see the game as a competitive task wherein the objective is to win, but females construe the game as a social interaction task with the objective of reaching an equitable and peaceable outcome.

All of these experiments demonstrate that performance depends upon how attitudes regulate behavior and that different individuals will cope with the same situation in different ways.

The second approach to relevance reveals the effects of differences in instigation. This point does not emerge, of course, in studies where kind of instigation is matched at the outset with either regulative or task conditions or both. Here, relevance is built into the experiment.

Differences in instigation are clearly revealed in an experiment by Atkinson and Reitman (1956). Subjects high in achievement performed significantly better than those low in achievement in an arithmetic task under achievement-arousal but not under a multi-incentive condition. However, the multi-incentive condition, in which "pleasing the experimenter" was a factor, proved to be especially favorable for persons high in affiliation and low in achievement. French and Chadwick (1956) have further shown that conditions designed to arouse affiliation are especially effective for individuals high in intrinsic affiliation. French (1955) has emphasized the same point with respect to achievement.

Going a step farther, Moulton *et al.* (1958) have shown that persons high in achievement are especially sensitive to achievement-related material under achievement arousal, and Atkinson and Walker (1956) find that persons high in affiliation are especially sensitive to affiliation-related material under affiliation arousal. These assembled results lead to the hypothesis that persons who differ in intrinsic instigation properties may respond differently in the same situation parallel to the point previously made for regulation.

Several investigations support this hypothesis. Chaney and Vinacke (1960) found important differences in strategy between members of triads high in achievement compared to those high in nurturance. Vogel *et al.* (1959) demonstrate not only that physiological reactivity is highest when instigation (achievement or affiliation) is relevant to the task conditions but also that performance in perceptual-motor and scrambled-word tests is superior under the relevant, as compared to the irrelevant, conditions.

French (1958) composed groups of four persons, each group high in either achievement or affiliation, and had them reconstruct stories under either individual or group orientation and with either a "task" feedback emphasizing the efficiency and details of work or a "feeling" feedback stressing the interaction situation. The achievement groups were more efficient with task feedback under both orientations. The affiliation groups were most efficient under group orientation with feeling feedback.

In an ingenious experiment by Schönbach (1959), hungry subjects engaged in a relevant task thought about food significantly more and also made significantly higher estimates of time delay prior to obtaining the goal object (food) than did hungry subjects in an irrelevant task. The same kind of relation occurred with a reversal of instigation and task.

In these two approaches to the problem of relevance, we see clear evidence that behavior depends upon how the various levels of motivation fit together in a given task situation. Only by differentiating motivational variables in some manner like that attempted in this paper can we hope to gain understanding of the conditions that determine cognitive processes. Preliminary steps

in this direction suggest that the principle of relevance can serve as a valuable integrating concept. I propose that both the quality and quantity of thinking vary with the relevance of task conditions to the motivation system. Performance should be at its best when instigation, regulation, and adjustment are all relevant to the goal.

Research to deal with these complexities will not be easy. Because the patterning of the motivation system is a function of relations among so many kinds of variables, a reorientation is demanded. Attention must be shifted from preoccupation with situational variables to the properties of the subject with whom the experimenter is concerned.

Experimentally, this means manipulating relations among motivational variables in such a manner as to permit differences in relevance to operate. Thus variations in instigation must be related to variations in attitudes, and these relations in turn studied under varied task conditions. As we have seen, research is rapidly pointing in this direction.

Another possibility is to start with the subject himself and thus develop a frankly "case history" approach to thinking analogous to the tactics of Smith *et al.* (1956) in their investigation of opinion. The objective would be to move from an understanding of why particular persons think as they do toward the identification of more general patterns of relevance. For example, two samples of persons could be used, one characterized by high achievement scores and the other by high affiliation scores. Their attitudes could then be explored by a variety of means. After that, these subjects could be exposed to an assortment of tasks (imaginative as well as problem-solving) under varied inductions so that relations among instigation-regulation patterns and task conditions could be identified. It is possible that each case would prove to be unique, but it is also quite possible that considerable insight would be gained, leading to the formulation of exciting new hypotheses about thinking as a function of the interaction between motivational and situational variables.

Conclusions

I have attempted in this paper to show that the onward course of thinking is determined by the organization of motive systems in relation to the environmental conditions that confront the individual. The development of principles to account for these complexities is, however, still in its infancy. Current research indicates that the following points need to be given special attention.

It is necessary to distinguish intrinsic properties of motivation from task conditions. The total system may fruitfully be conceptualized in terms of the levels of arousal, instigation, regulation, and adjustment.

Effort directed towards discovering how these levels are organized under varied task conditions would be more productive than manipulating variables at only one level.

Specifically with respect to thinking, research is needed in the following areas: (1) measurement of qualitative as well as quantitative changes associated with variations in motivation and environmental conditions; (2) comparison of complex with simple tasks; and (3) comparison of motivational properties defined by pretesting, deprivation, and experimental induction.

A reorientation is demanded with attention directed towards the subject

instead of preoccupation with the situation itself. This may be accomplished by explicit experimental recognition of the problem of relevance or by adopting a case history approach, with research proceeding from an exploration of personal motivation systems and their relation to task situations to hypotheses concerning patterns of relevance.

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EXPERIMENTAL STUDIES OF THE ROLE OF VERBAL PROCESSES IN CONCEPT FORMATION AND PROBLEM SOLVING

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To speak of the role of verbal processes in concept formation and problem solving involves essentially a reexamination of the (John) Watsonian and (Benjamin) Whorfian hypotheses concerning the role of language in cognitive functioning. Watson said that thought largely consists of implicit responses. He emphasized implicit verbal responses but did not ignore others. Other writers, in interpreting Watson, have tended to restrict their attention to the role of subvocal speech in the process of thinking and have based their acceptance or rejection of his view on this basis. Whorf espoused a doctrine even more radical in certain ways than Watson's. It was Whorf's idea that the structure of a language, as well as its words, determine not only what and how we shall think but also how we perceive the very world with which so much of our thought is concerned. Neither Watson nor Whorf was the first to advocate hypotheses relating thought to language, but the work of the present day owes perhaps more to them than to their predecessors. Watson's influence reaches to the present through his behavioristic and neobehavioristic successors, Whorf's through anthropology and linguistics. Watson's influence probably pervades much of the study of concept formation and problem solving in the psychological laboratory and of the study of mediational processes generally. Whorf's views have gained their greatest interest from the psycholinguisticians.

It is possible, following Meehl's (1950) example, to speak of a strong and a weak form of the Watsonian and Whorfian hypotheses, and it seems to me that the present evidence probably is consistent only with weak forms of these hypotheses, that is, that much thought involves or, if you will, *is* verbal process and that the form and content of a language probably make some kinds of thinking and perceiving easier in one language community and harder in another. The reasons for not espousing strong forms of these hypotheses are these. I suspect that preverbal children and animals think. This, of course, depends on how we define thinking but, for example, I believe the delayed reaction probably qualifies as thought. It is said by at least some linguists that anything that can be said in one language can be said in any other, somehow. These points would seem to defeat the strong forms of these hypotheses. I do not think this is very important: there is plenty of room for verbal processes to affect thought significantly within their weak forms (cf. Revesz, 1954).

Some years ago, Lloyd Morrisett called my attention to a case that can have a tempering effect on enthusiasm for the strong forms of these hypotheses. Smith *et al.* (1947) were interested in the question of what central depressing or analgesic properties are possessed by the drug Curare. Smith volunteered to be a subject of a study of this problem. A form of Curare (*d*-tubocurarine) was administered to Smith intravenously; 500 units were given over 54 min. Skeletal muscle paralysis was complete, and oxygen and artificial respiration

were required. The subject, of course, after a time, was able to make no gestural or vocal responses. His report, dictated after recovery, was that he was "clear as a bell" and knew what was going on; his recall for the things said or done to him during the period of total paralysis was reported to be excellent. He was apparently able to solve simple problems so long as there was any means of communication with him, for example, by a thumb gesture after speech was gone. The EEG was normal throughout and responded appropriately to pattern vision. I fully recognize the limited conclusions that a study like this permits, but the strong forms of our hypotheses, it seems to me, require some evidence of intellectual disturbance under conditions like these.

Let me turn now to a consideration of studies that offer support to the weak forms of these hypotheses. First, there are those involving cultural or language differences. Several studies have been concerned to show that in nonwestern, relatively primitive societies abstract behavior nevertheless exists (McConnell, 1954; Jahoda, 1956). Typically, such studies have not explored analytically the relation of language to abstract behavior, although Jahoda suggested that literate children from the Gold Coast (now the nation of Ghana) in Africa did better on the Goldstein-Scheerer cube test than did illiterate children. Mead (1932) reported no evidence for the existence of animism among Manus children. There are a number of problems in the interpretation of her results, among them one reflected in Jahoda's (1958, page 206) comment that the Manus language is "simple, containing hardly any metaphors or similes." Unfortunately, Mead's results do not lend themselves to an unequivocal explanation on this linguistic basis.

Lenneberg and Roberts (1956) compared monolingual Zunis, United States college students, and bilingual Zunis in their ability to recognize previously shown colors when these colors were later shown together with a number of others. In the Zuni language the colors orange and yellow have the same name and the monolingual Zunis often confused these two color chips, whereas the monolingual college students tended not to confuse them. Bilingual Zunis fall between these two groups in their color recognition performance. Carroll and Casagrande (1958) have presented a brief report from the Southwest Project in Comparative Psycholinguistics in which two experiments are described. One was carried out with 14 adult Hopi Indians and 12 comparably-educated adults in rural New England. The experiment consisted of presenting three pictures to a subject and asking him to decide which two go together. In the Hopi language there is a verb that means to close an opening, such as to put a cover on a box, and there is another verb that refers to putting a cover over something to protect it against dust or damage. In English the verb cover would probably be used for these two cases, and the verb close would probably be used to designate the act of putting a hinged lid of a box in place. One of the several sets of pictures used by Carroll and Casagrande was composed of the following pictures: (1) a woman closing the hinged lid of a box; (2) a woman placing a dust cover over an object; and (3) a woman placing a top over a wicker basket. The prediction was that the Hopis would put together pictures (1) and (3) that should be brought together through their verb meaning to close

an opening; they were not expected to put picture (2) with either of the others, since the other verb for covering something against dust would be appropriate there. The English speakers, however, were expected to put together pictures (2) and (3), the action of both of which can be described as covering something and were not expected to pair picture (1) with either of (2) or (3), since the action of (1) is best described as closing the lid of the box. Over a range of 17 such items there was, in general, support in the sorting data for these predictions from the linguistic analysis, but the differences did not reach a satisfactory level of statistical significance.

The second experiment reported by Carroll and Casagrande reached a more acceptable level of statistical significance. In this experiment Navaho children of ages 3 to 10 years were used, and they varied in linguistic status from monolingual Navahos to monolingual English speakers. In Navaho the verb form used to designate the handling of an object varies with the shape or other essential attributes of the object handled. Thus different verbs would be used to refer to handling a long piece of string, a long stick, or some flat, flexible material like a piece of paper. The Navaho children were shown a series of pairs of objects and were then asked, for each pair, to indicate with which member of the pair a third object belonged. For example, one pair consisted of a blue stick and a piece of yellow rope of the same size. The third object was a blue rope. Seventy-one per cent of the children who were linguistically Navaho-dominant chose the yellow rope as the object the blue rope went with best, whereas 40 per cent of the English-dominant Navaho children made this choice. This is a highly significant difference, and a number of the other items significantly differentiated the two groups as well. Presumably, the Navaho-dominant children put the yellow and blue ropes together because of the common verb with which their being handled is described, whereas the English-dominant Navahos tended more often to use color as the basis of grouping and put the blue stick and the blue rope together.

While these studies are suggestive that linguistic factors may affect recognition and sorting behavior, such research is at best difficult to do and the results are not always compelling. Lenneberg (1953) has argued that Whorf's hypothesis can be tested best within a single culture, and Brown and Lenneberg (1954) did so. They first determined for each of 24 Munsell color chips its "codability," that is, the amount of agreement subjects displayed in the names they gave it. Then a chart containing 120 colors, including the just-mentioned 24, was provided, and the subject was asked to select from among the 120 those colors that had just been exposed to him in a tachistoscope. A positive relationship was demonstrated between the codability of a color and the subject's ability to find and recognize it on the color chart. This was the same task that Lenneberg and Roberts used in their study of Zuni Indians, which I mentioned above. Hunter and Ranken (1956) also reported that color-sorting behavior could be mediated through the names taught the subjects for relatively unfamiliar colors. A negative note in this series of studies is sounded by results obtained by Flavell (1958). He showed two identical pictures briefly by means of a tachistoscope after the subject had seen in the fixation field a pair of titles, one for each picture. One title was a verb, the other a noun. Flavell asked his subjects to judge which of the two pictures showed more movement or

action. He expected the picture titled by the verb to be judged more active than the other one, but his results showed no difference.

A good deal of attention has been paid in recent years to the role of verbal processes in rather more standard concept-formation situations. Several kinds of studies have been carried out. In one type the subject has been taught a verbal response that can mediate concept formation. Another has studied characteristics of the instances that are often themselves verbal, with which the subject works in order to establish the concept. Also, there are a number of other studies, not so easily classifiable, that have, however, relevance to the general problem.

A series of experiments by A. E. Goss and his students and a series by H. H. Kendler and his associates illustrate the first kind of study. Goss' studies employed a block-sorting task, similar to the Hanfmann-Kasanin sorting problem, in which a combination of two properties of the blocks was the basis for correct sorting. Prior to the sorting task itself the experimental subject was given training in which he learned a name or nonsense syllable in association with the blocks. Suitable control groups were employed. Fenn and Goss (1957) found with this procedure that both normal and paranoid subjects performed the sorting task much better than the controls after learning the name or nonsense syllable for the blocks, and Carey and Goss (1957) found a similar effect with preschool children in the case of the names. Goss and Moylan (1958) varied the degree of mastery of the name-block and nonsense syllable-block associations. They found, with college students, that degree of mastery had little effect on sorting when the names were being used, but that adequacy of sorting behavior was related to degree of learning of nonsense syllable-block associations. Lacey and Goss (1959) varied the number of nonsense syllable labels learned to the height-size category in presorting training of college students and also varied the degree of mastery of the nonsense syllable-block association. Again demonstrating an effect on sorting behavior of degree of mastery, they also showed that it was more effective, generally, if the subject learned three or four labels within the height-size category than if he learned only two or none. Goss (1958) has presented a thorough analysis of the role of mediating responses in concept formation.

H. H. Kendler (1960) has recently summarized the series of studies that he, T. S. Kendler, and their associates have made. As Kendler's paper has summarized this material fully elsewhere in this monograph, I shall not discuss it here.

These direct demonstrations of the role of verbal responses in concept formation could be buttressed further by a summary of indirect evidence from various studies of transposition in discrimination problems. For example, Spiker *et al.* (1956) found that children who, on test, demonstrated that they understood the notion of middle-sizedness performed better on a relational task involving this concept than children who did not understand the concept (Spiker and Terrell, 1955). Unfortunately, transposition studies of this type do not always yield results consistent with the indirect assessment of the presence of the verbal label. The papers by Stevenson and Iscoe (1955) and Gonzalez and Ross (1958) are consistent with this point.

A number of experiments have appeared recently in which response-eliciting

properties of the instances of the concept have been related to performance. Some of these have arisen from E. Heidbreder's argument that there is an order of difficulty in the emergence of concepts, the concrete or thing concepts being easiest, number concepts most difficult, and spatial concepts intermediate. Baum (1951) presented some evidence that ease of concept attainment is inversely related to the number of different verbal responses the instances elicit. Deterline (1957) obtained verbal responses to stimuli from each of Heidbreder's concept classes and found decreasing agreement between the stimulus- and response-concept classes as he went from object to form and to number. The implication is that the differential difficulty of the concepts is due to the different numbers of competing responses they evoke rather than to their variation in abstractness. There is probably a parallel here between the color codability measure that was used in the work of Brown and Lenneberg; stimuli that are highly codable in an appropriate concept class lead to easier concept formation than less codable stimuli. A somewhat similar interpretation can be made of the results of several studies that have used Underwood and Richardson's (1956*a*) materials. These materials consist of words to which sensory associations were made by college students. Each word thus has a score or scores based on the percentage of subjects "agreeing" in associating similar responses to it. Underwood and Richardson (1956*b*) compared concept learning for three kinds of concepts: one in which the words or instances had a high dominance score; one in which the dominance score of the words was low; and one in which intermediate dominance was present. For example, the word "small" was elicited by each of the four words belonging to this concept in a high proportion of the subjects; this would be a high dominance concept. In the low dominance set, no word elicited the sensory word in more than a few of the normative sample. As would be expected, the concepts were easiest to attain in order from high dominance, intermediate dominance, and low dominance. Freedman and Mednick (1958), using the Underwood-Richardson materials, compared concept learning for the following kinds of concepts. The word "gnat" elicits the response "small" 76 per cent of the time, whereas the words "needle," "stone," and "canary" elicit it from 5 to 9 per cent of the time. This is a high variance concept. Another concept, a low variance concept, is provided by the words "sauerkraut," "hospital," "tobacco," and "gym," that elicit the sensory word "smelly" from 21 to 24 per cent of the time. The average dominance of these two concepts was about the same. The high variance concept, however, was easier to learn. Griffith *et al.* (1959) asked normal and subnormal children to find a similarity among three words, like coffee, tea, and cocoa. They were also asked, at another session, to define each of the words in such triads. In general, the percentage of correct abstractions, that is, similarities, achieved increased as a function of the number of word definitions given in which common abstractions figured. Forgus and Fowler (1957) had their subjects learn thing, form, or color concepts varying the familiarity of the instances. The concepts based on familiar instances were learned most readily.

The nature of the responses elicited by the concept instances then seems to be an important factor in the attainment of verbal concepts. A parallel finding exists in the phenomenon of clustering in free recall. Bousfield (1953) pre-

sented a long list of words once to his subjects and asked for an immediate recall. The words in the list were presented in randomized order but belonged, in fact, to four mutually exclusive categories, like animals, occupations, vegetables, and weapons. In their recalls the subjects tended to emit words in groups or clusters from the same category. Bousfield and his associates then did a normative study in which associations to a variety of category names were obtained. (Cohen *et al.*, 1957). It was then possible to set up a list in which the words were frequent associates to their category names and another one in which the words were infrequent associates to the category names. Much more clustering was shown for recall of the frequent associates list than for recall of the infrequent associates list (Bousfield *et al.*, 1958), a result confirmed by Holroyd and Holroyd (in press) and by Cofer and Segal (1959) independently. This recall task is probably a concept-formation situation, and it would seem reasonable to equate these results with those from the concept dominance and codability studies just mentioned.

The nature of the verbal form of the instances has been studied in relation to verbal concept formation. Heidbreder and Zimmerman (1955) found with verbal phrases that if nouns designated the critical concept features, concept attainment was easier than if adjectives designated the critical features. Ross and Levy (1960) discovered that the categories designated by two antonyms may or may not be equivalent. For example, having subjects arrange cards into "beautiful" patterns produced very few patterns, whereas having them arrange the cards in "ugly" patterns produced many more patterns. It follows that conceptual behavior mediated by such antonyms ought to differ. Verbal concepts may encompass different words as a function of personality variables or of experience. Mayzner and Tresselt (1955) found that subjects who scored high on a given Allport-Vernon value would include more words in that conceptual or value area than would subjects who scored low on the value area in question. They also found that having subjects allocate unambiguous words to value categories would markedly reduce the number of ambiguous words subsequently allocated to the same categories (Mayzner and Tresselt, 1958*a*, 1959*a*). Other things equal, however, the categorizing of verbal material into value or conceptual areas is remarkably consistent over time and with different methods (Tresselt and Mayzner, 1958, 1959).

I have now indicated that close relationships exist between aspects of verbal behavior on the one hand and conceptual behavior of various sorts on the other. Except for the color recognition, sorting, and discrimination experiments mentioned earlier, much of this work has concerned verbal concepts. Perhaps, therefore, it is not surprising that verbal characteristics or responses are involved in verbal conceptual behaviors. Brown (1956, 1958*a*, 1958*b*) has argued that one of the major functions of language is conceptual, that is, to help categorize the world of varying objects. I think that this is probably true. Thus it is appropriate to deal with verbal concepts that of course reflect more or less directly aspects of the language. Various other experiments can be cited to show the ordering functions of verbal behavior in both nonverbal and verbal situations. Davidon (1956) showed that in grouping of names the number of names that subjects could put into a group was larger than the number of pictures of objects they could put into groups, suggesting that the

names are less specific and less concrete, perhaps, than are the objects. Brown (1957) found that the grammatical form of a nonsense word could be determinative of the aspect of a picture that a preschool child would identify as having been referred to by the nonsense name. This indicates a role in the organization of the world for formal as opposed to contentual aspects of language as well as suggesting that children learn something of the characteristics of grammatical classes at a very early age, at least in a functional sense. Matthews (1954) and Helson and Cover (1956) have shown that recall of names is affected by the kinds of categories to which the names are assigned. The role of class descriptive cues as compared to specific cues has been shown to be a significant one in paired associates learning by Wulff and Stolurow (1957) and in a transfer task that measured retention by Lloyd (1960*a*, 1960*b*).

We also find studies of verbal processes in the kinds of experiments usually labeled problem solving. There may be no fundamental difference between problem solving and concept formation, but there are usually differences in experimental operations. Spiker (1956) found that teaching his preschool subjects distinctive names for the stimuli used in a delayed-reaction situation significantly improved their performance over suitable controls. His younger subjects benefitted most from the distinctive names, a finding paralleling that in Kendler's report for his younger subjects in a reversal-shift situation. Gelfand (1958), adapting a method previously used by Judson *et al.* (1956), taught his subjects word lists before asking them to solve a problem; the word lists were either relevant, irrelevant, or neutral with respect to the dimension involved in the problem itself. The group that learned the relevant word list performed better than the other groups on the problem, especially on the most complex level of the tasks performed. Saltz and Newman (1960) found that some learning of the names of components facilitated performance in an aspect of the task of assembling a pressure regulator. Ray (1957) showed that if the subject had to tell the experimenter ahead of time what he was going to do, his performance of a manipulative light-switching task was superior to that of subjects not required to discuss their performance. Hafner (1957) in a similar experiment with children, asked one group to verbalize their thoughts while doing a test. This group made fewer moves in performing the task than the controls, although other measures did not differentiate the two groups. Staats (1957) expected to find that subjects who associated weight responses to a screwdriver would do better on a modified version of the Maier two-string problem than subjects who did not associate weight responses to it. The anticipated relationship did appear but not for the preproblem associations; it was true only when the associations were made after the problem was attempted.

The other major research of a problem-solving character that has come to my attention in the recent literature has concerned word problems. Mayzner and Tresselt (1958*b*, 1959*b*) found that solution frequency of the ever-popular anagram problems is related to the word-frequency of the word to be found, the extent to which the letter-order of the anagram resembled that of the word, and the transitional probability of letter sequences in the anagrams. Low frequency words, high transitional probabilities of the anagram letters, and little resemblance to the word letter-order made anagrams difficult. It is un-

known, at present, how these findings might affect the interpretation of the many studies that have used anagram problems in the past. Battig (1957, 1958) has worked with a word-formation problem. The subject is asked to identify words by naming letters until he gets all of the letters of the word in question. The subjects who did well in this task tended to guess the letters in the order of their frequency of usage, while the poorer subjects were more likely to guess in terms of alphabetical sequence. Previous experience and knowledge about letter distributions facilitated this performance. Rhine (1959) asked his subjects to solve both spatial problems and anagrams. He identified among his subjects those whose anagram performance showed letter hypotheses on the one hand and whole-word hypotheses on the other. He found that the subjects who made letter hypotheses did better not only on the anagram problems than those who made whole-word hypotheses, but also on the spatial problems. These experiments suggest that a kind of fine grain aspect of verbal behavior is significant to this kind of problem solving and also suggest controls that may be necessary in further use of these kinds of problems.

I have not spent much time on verbal factors in problem solving because I have not turned up a great deal of relevant research, at least of a direct kind, in the period under review. Much more attention has been paid to concept formation.

This review of the work on verbal processes in relation to concept formation and problem solving has indicated that often relations can be demonstrated. At the present we do not know how prevalent they are or what the conditions and mechanisms are under which and by which they function. I had thought of introducing into this paper a consideration of semantic generalization, mediated generalization and transfer, stimulus predifferentiation, and acquired distinctiveness studies, because these areas of inquiry relate also to the mediation hypothesis that is implicit in most of what I have said already. Further, they probably get directly at processes such as those involved in concept formation and problem solving. However, space does not permit me to go into these matters and, as a matter of fact, the conflicting results that have appeared recently in almost all of these areas make it impossible for a summary statement to do them justice. Instead, I shall turn to a brief survey of some recent work in verbal behavior itself. This work has, I think, considerable relevance to thinking because if, as I have suggested, thinking is closely related to verbal processes, advances in the study of thinking may come from the study of verbal behavior, especially verbal associative processes. There are several matters that I shall discuss. In general, they are concerned with factors that affect the associative responses or associative relations that subjects can bring to a problem-solving or concept-formation task. Heretofore I have emphasized the making of a specific mediating response, as in the Goss and Kendler studies, that may orient the subject to the appropriate dimension of the problem, codability of stimuli as a basis for color categorization and concept formation, and the transfer of specific associations in the facilitation of problem solution. I now consider context factors in the association experiment, the problem of "priming" of associations, the bidirectional nature of word associations, and Maltzman's work on originality training.

We are accustomed to think of word associations to verbal stimuli as being

relatively stable responses, and it is perhaps problematic that such associations are often proposed as the basis for what we like to think of as original or creative thinking. It is certainly true that under specific conditions the associative responses given to stimuli by comparable groups of subjects or by the same subjects at different times are often stable. However this stability depends on relative constancy of conditions. Howes and Osgood (1954) were the first to show, I think, that placing a word in a context of other words considerably changed the responses that subjects gave to it. Jenkins and Cofer (1957), using adjective-noun combinations, found that the responses given to the combinations often showed virtually no overlap with the responses given to the noun or to the adjective alone. In their study, such overlap never exceeded 40 per cent (cf. Musgrave, 1958). In a later study, Gonzalez and Cofer (1959) found that suitable arrangements of modifier-noun relationships could facilitate or inhibit the occurrence of clustering in the free recall of nouns, and Cofer (1960), reporting an associative study of the combinations used by Gonzalez and Cofer, has shown that most of these effects on clustering parallel changes in the associative response distributions occasioned by the pairing of the modifier and its noun. Contextual effects then, on the basis of present evidence, can powerfully influence the sorts of associations that given verbal stimuli (and probably nonverbal stimuli) can arouse. The systematic significance of these findings to problem solving and concept formation is clear only to the extent that we might expect a variety of associations to stimuli in the context of the problem that we would not have had to these stimuli in the word-association test.

A closely related phenomenon is that of "priming" of associations, which Storms (1958) first investigated and called a "recency" effect. I can illustrate his procedure with an example. The word "hill" is a high-frequency association to the word "mountain," but "mountain" is not a frequent association to "hill." What Storms did was to ask his subjects first to learn a 14-item list of words, composed of words like "mountain." Then he administered a free association test in which 14 of the stimuli were, like "hill," frequent responses to the words of the preceding list but that were not stimuli that ordinarily elicited the words of the preceding list. The results, however, showed that after these operations the previously-learned words now occurred with some frequency as responses to the words to which ordinarily they did not occur. Thus, "mountain" would now be given as a response to "hill," whereas, as I have said, it does not ordinarily so occur. Sydney Segal, working at New York University, New York, N. Y., has verified these findings of Storms and has shown that a similar, although perhaps not as extensive an effect, can be shown to occur when the initial list of words is incidentally rather than intentionally learned. Strongly suggested by these findings is the idea that there are many "latent" associations that can be activated by suitable arrangements of experience. It is not impossible that similar processes can operate during the course of problem solving and that subjects may differ in their susceptibility to the priming operation, (cf. Maltzman and Simon, 1959).

Another striking effect demonstrated by Storms (1958) is that, in mediated transfer situations, backward associations may be facilitative of transfer just as are forward associations. Let *B* stand for a Kent-Rosanoff stimulus word

and *C* stand for its frequent response. *B*, however, does not occur as a response to *C*. Let *A* stand for a nonsense syllable. What Storms did was to see whether the learning of *A-C* pairs would facilitate the learning of *A-B* pairs, and he found that facilitation did occur. He and Russell (Russell and Storms, 1955) had already shown the facilitating effects of forward associations in a parallel design, so that these word associations can apparently work bidirectionally. Phebe Cramer, working at New York University, has repeated and verified this experiment of Storms and has extended it by studying all eight of the possible relations among the three terms *A*, *B*, and *C* in the two lists. She finds facilitation of second-list learning in every case. This work involves existing associations, and the results are much clearer here than they are when parallel experiments are run with nonsense syllables. Under some conditions, then, associations can work both ways, and this probably adds a flexibility factor to the role of such associations in thinking. There are, by the way, certain cases in which backward associations, even of the word-word kind, do not show transfer, and it is very likely that where they work it is priming, or possibly an implicit learning of their backward order, that are the reasons they work. However, in prediction of some kinds of transfer from measured associations, the bidirectional possibility must always be entertained. It is worth adding, I think, that Jenkins (1959) has shown that subjects can increase the number of popular associations they give to an association test if they are instructed to do so. This is probably further evidence that many associations are latent in all of us, to be evoked by context, priming, instructions, and probably many other factors.

The work of Maltzman and his associates with originality training is probably consistent with this conclusion.* This work has used, typically, a free-association test and the Guilford Unusual Uses Test, which requires subjects to think of uncommon uses for ordinary objects. One procedure has been to repeat the same 25-item association test under instructions to give a different response each time to each stimulus. Another procedure has been to ask the subject to give the same response to each stimulus on successive presentations of it. A major finding is that giving different responses to the same stimulus words on successive presentations of the list markedly increases the number of unusual or uncommon responses on both another association test and on the Original Uses Test. On the other hand, repetition of the same response to the same stimuli, or reading uncommon responses or uncommon stimulus-response pairs, does not change the originality of the responses to the subsequent free-association test or to the Uncommon Uses Test. Originality training with the Uncommon Uses Test produces greater originality later on another version of this test, but does not affect the later free-association test. Instructions to be original produce some effect on originality in the free-association test, but not as much as these instructions together with practice in making different responses to another free-association test. The amount of originality training is related to the originality score obtained on both the free-association and the

* This discussion is based on a paper by Maltzman *et al.* (1958) and on five technical reports prepared by Maltzman and his associates under contract NONR 233(50) between the Office of Naval Research, Washington, D. C., and the University of California, Los Angeles, Calif., dated August 1958 and August 1959. Publication of these reports is expected soon.

Uses Test, and some effects of this kind of training persist for at least two days. I have not done justice to Maltzman's work in this short summary, but it seems to be clear that practice on the part of the subject in making different associative responses to the same verbal stimuli somehow makes it possible for the subject to be more original, not only on another set of association stimuli, but also on another task, the Original Uses Test. This work further demonstrates the potential flexibility and availability of associations when conditions are established that bring out such features.

In summary and conclusion, I have reviewed cross-cultural data, experiments on the mediating role of verbal responses in categorization, sorting, and certain discrimination and other concept formation situations that have revealed that such verbal responses can influence characteristics of performances in such circumstances. Associative responses to conceptual instances, variously called codability or dominance, and the verbal form of concept instances have been shown to affect concept formation. Verbal concepts are consistent over time, but their scope or breadth bears relations to personality variables and to the frame of reference established by prior experience in which they are assessed. Object identification, memory, and learning have been shown to be affected by grammatical class and conceptual categories. Problem solving can be influenced by associative patterns among words pertinent to the problem and by vocalization of the steps taken to work on the problem, and solution of verbal problems has been shown to be a function of general linguistic properties like word frequency and letter sequences. Such evidence supports the notion that verbal processes are closely interwoven with much problem solving and concept formation so that the weak forms of the Watsonian and Whorfian hypotheses are supported. I have further indicated that context and priming effects on word association, bidirectional effects of word associations, and the originality-training procedures just described seem capable of significantly altering the kinds of associations subjects will make to verbal stimuli.

I have refrained from discussing theoretical issues, although the very topic I have discussed has implied throughout commitment to a two-stage S-R conception involving mediating responses. Such theoretical development as has been achieved in this area is already well known and hardly requires further explication here. On the other hand, conflicting results recently obtained in several pertinent areas of experimentation, together with the recent results of studies of verbal behavior that I summarized at the end of this paper, cause me to be concerned that our past conceptions of mediating processes are too simple. I think we need more data to theorize meaningfully. Despite the fact that I have discussed at length research results, I still feel the following to be an inescapable conclusion: We know so little.

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Part IV. Creativity and Thinking

THINKING AND CREATIVITY*

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In his letter of invitation, Ernest Harms suggested that this paper "should emphasize the major present problems and tasks for the future" in research on creative thinking. To do this it will be necessary to deal briefly with each of several important topics. Results from individual studies will be cited to illustrate particular points, but it will not be feasible to summarize fully the results of any given study. Nor will it be feasible to cite all of the studies relevant to any given point. I trust that those whose work is directly relevant but unmentioned will understand that limitations of time make it quite impossible to make even brief reference to all of the empirical research that falls within the scope of this paper.

The initial problem in research on creative thinking is that of definition, for there is no one generally accepted definition of the term "creative." My own research during the past 10 years has included primarily studies of problem solving but also studies of creative thinking as well as of decision making. In my view all three of these areas of investigation fall within the more general area of thinking. Guilford (in this monograph) argues that problem solving is not a unitary process; I am inclined to agree, although perhaps on the basis of different criteria as to what is unitary. Problem solving is best defined, not in terms of process for it involves a variety of processes, but in terms of product: problem solving is that thinking resulting in the solution of problems. Similarly, decision making is that thinking resulting in choice among alternative courses of action.

My view is that creative thinking is also best defined not in terms of process but in terms of product. Examination of a variety of proposed definitions of creativity indicates that although there is disagreement, common to most of them are the ideas of novelty and worth. Novelty or originality is ordinarily regarded as a necessary but not a sufficient condition; if what is new is to be regarded as creative, it must also be of value by some criterion. Creativity may best be defined as that thinking which results in the production of ideas (or other products) that are both novel and worthwhile.

Others would question this viewpoint and prefer to define creativity in terms of process. Stein (1956), for example, has written: "In general one tends to judge the creativity of others in terms of the 'products' that they have produced. . . . Such an orientation leads us to overlook the fact that creativity is a process. It is a process of hypothesis formation, hypothesis testing, and communication of results. Creativity may be manifest in any one or all of the aspects of this process." It may be noted, however, that he later added (Stein, 1956): "For purposes of empirical research our definition of creativity is as follows: Creativity is that process which results in a novel work that is accepted

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as tenable or useful or satisfying at some point in time." The view I would take is that the question of what processes are involved in creative thinking and the question of whether these are the same as or different from the processes involved in such thinking as problem solving and decision making are questions to be solved not by definition but by empirical investigation. Some attention will be given to these questions later in this paper. It may be appropriate at this point to recall Guilford's warning of the danger of assuming that one name means one process.

Research on creativity may focus upon: (1) products, (2) processes, (3) tasks, (4) persons, and (5) environmental variables; or upon some combination of these. Attention may be devoted primarily to: appraisal of the products that result from creative thinking; exploration of the processes involved; examination of the influences of the characteristics of the problems or tasks undertaken; investigation of the relations between characteristics of the person thinking and the results achieved; or study of the effects of a variety of environmental variables upon creative thinking. These five represent the major problem areas, I believe, both now and for the foreseeable future. They obviously closely interrelate, and the discussion of any one will necessarily overlap somewhat with that of others. Nevertheless, it appears preferable to deal with them one at a time.

Products

Perhaps the largest number of studies completed thus far have been those concerned with the identification of those characteristics of the person that are related to creativity. Such studies, however, require a criterion of creativity: a procedure for measuring the degree of creativity shown by an individual or at least for distinguishing the more creative from the less creative. It has been in the effort to develop such a criterion that psychological research has focussed upon the products of creative thinking.

One result of such work has been to reveal the variety of products with which the investigator may be confronted within even one general area of creative work. McPherson (1956), for example, points out that among the usual creative products produced by a scientist are the following: patents, patent disclosures, publications, unpublished research reports, unprinted oral presentations, improved processes, new instruments, new analytical methods, ideas, new products, and new compounds. To be sure, common to all of these is the production of new and worthwhile ideas. Nevertheless, the differences among these items appear to be of importance for any systematic effort to appraise such products. Note that a much larger variety of products would be included if all fields of creative endeavor were to be included.

In appraising the creativity of individuals, the process has been sometimes employed of simply counting, without reference to their quality, the number of products of a given kind produced by each person. Employed with an acute awareness of its limitations, this procedure may yield results of value. However, the limitations of simple quantity as a criterion deserve emphasis. Work by McPherson (1956) and an unpublished study by myself would seriously question, for example, the use of number of patents as a measure of creativity even among individuals in the same general area. Many creative ideas are not

patentable; there is a wide range in quality among patents; the number of patents obtained varies markedly from one field to another; one individual may be motivated primarily toward science for science's sake and another may be what was referred to in one laboratory in which I worked as a "patent hound." In that laboratory my attention was called to one man who had over 200 patents but who in the judgment of his colleagues was less creative than another individual who had only two patents.

Similar limitations could be pointed out with respect to the use of quantity of other products as a criterion such as, for example, number of published papers. In a study of *America's Psychologists* (Clark, 1957), a careful nominating procedure was employed in identifying the 50 most significant contributors among those who had obtained their Ph.D.'s in each of three five-year periods. A comparison was made between the number of articles published by each of these groups of 50 and the number published by other highly visible psychologists who had obtained their degrees during the same period. Though the median number published by the significant contributors was larger in each case, the overlap between the two was "very substantial." Evidence is presented in the same study indicating that a more useful criterion may be the number of times an individual's published work has been cited by others.

Given the limitations of simple quantity as a criterion, the appraisal of the creativity represented by various products becomes an important problem: one that requires more work. Thus far there is no adequate substitute for the judgment of informed individuals in making such appraisals. This statement is made with full recognition of the fact that such judgments change over time. The painter neglected by his contemporaries may be acclaimed by the critics of a later day. Judgments about the significance of ideas not only change over time; they may in some areas be shown rather objectively to have been clearly wrong. Just how wrong they can be, we are reminded by Schmookler (1957) who in a study of inventors, discusses this problem and calls attention to the following statement by Robert A. Millikan in his book *Science and the New Civilization* published in 1930 (page 163; italics in text): "Einstein's equation and Aston's curve alone . . . enable us to draw one definite and very important conclusion, namely, that *there is no energy available to man through the disintegration of any of the common elements.* Man will presumably some day learn to disintegrate the elements, but he will have to expend energy upon them to do it. *There is no appreciable energy available to man through atomic disintegration.*" Judgments by "experts" as to the significance of, or degree of creativity represented by, ideas or other products may often be in error. However, there is no real alternative available to us in the appraisal of products of creative thinking.

Some data are available to indicate that judgments of the quality of ideas can be made with reasonable reliability. For example, I had judges (1960*b*) independently appraise such characteristics as the feasibility, effectiveness, and generality of ideas produced in an experiment on creative thinking (Taylor *et al.*, 1958). The correlations between two raters working independently varied from 0.47 to 0.83 with a mean (via *z* transformation) of 0.64. Correlations from 0.41 to 0.85 with a mean of 0.68 were obtained between ratings by three different raters and ratings by the same judges repeated two years later. Since each judge originally rated more than 1700 items, it is highly doubtful that

memory after two years could have contributed any spurious reliability. These were the reliabilities of the ratings of individual ideas. Each individual or group, of course, produced a large number of ideas; the score employed was the sum of the ratings of these ideas. The reliability of such scores would be expected to be higher than that of the ratings of single items, just as the reliability of a test is considerably higher than the reliability of the individual items of which it is composed.

Lest we become too sanguine about the reliability of such judgments, however, attention should be called to another study. Shaycoft and Altman (1955) employed a careful procedure in obtaining appraisals of 60 M.A. and Ph.D. theses in the fields of chemistry, chemical engineering, and civil engineering. For each thesis, one appraisal was made by the chairman of the thesis committee; the other was made by an evaluator who did not know the students. The median intercorrelation between independent evaluations by the two evaluators was only 0.18. Much more work is needed on the development of procedures for appraisal of products of creative thinking.

It is quite important to note that the judgment as to whether a given idea or other product is novel and worthwhile may be made with respect to either of two frames of reference, one social and the other individual. Employing the social frame of reference, it may be asked whether what has been produced is novel and worthwhile in terms of the present state of knowledge. Employing the individual frame of reference, it may be asked whether what has been produced is novel and worthwhile in terms of the life history of that particular individual. A given product (or individual) may be judged as uncreative in terms of the former, and as highly creative in terms of the latter, criterion. Thus, for example, suppose that a person in some isolated culture had acquired from others no knowledge of mathematics beyond arithmetic; suppose that independently he succeeded in inventing the calculus. By the social criterion this product (and individual) would not be regarded as creative, for nothing new has been added to knowledge. However, judged in terms of the individual's own life history, the same product (and individual) would be regarded as highly creative.

Assessments made in terms of one frame of reference are in some sense incommensurate with assessments made in terms of the other. Nevertheless, both kinds of assessments have been used and are needed in research on creativity. Of the two, the social criterion is probably much more frequently employed; it is probably easier to use and may well be more reliable; for those interested in determining the variables related to actual additions to knowledge, it may be the more relevant. However, the case can be made that, for the psychologist focussing on process, the individual criterion is the more relevant. In any case, use of the individual criterion seems particularly appropriate for those interested in studying creativity among children.

If creativity is defined in terms of the production of new and worthwhile products, then assessment of the creativity of individuals must be closely tied to the assessment of products. Essentially it must be asked: To what extent has this individual produced new and worthwhile ideas, judged either in terms of a social or an individual criterion? This approach involves rejection of the view that an individual may be judged as creative even if he has produced

nothing new, provided that he exhibits certain processes or characteristics deemed for some reason to be associated with creativity.

In many studies judges are asked to rate individuals in terms of degree of creativity. Although such judgments usually involve no explicit assessment of particular products, those selected to make the ratings are typically persons who have considerable knowledge of what the individuals to be rated have produced in the past. Evidence available indicates that such ratings can be made with useful reliability. In a study that I did two years ago (1958), I obtained ratings by immediate and secondary supervisors for about 100 men in a research and development laboratory on a check-list rating scale. Six months later I obtained ratings for the same men by the same supervisors on a second and quite different form, a descriptive scale. The intercorrelations between the ratings made six months apart on different forms was 0.71 for the immediate and 0.78 for the secondary supervisors; it was 0.83 for the mean ratings for each individual by the two supervisors. Sprecher (1959) obtained from peers as well as supervisors ratings of a large number of engineers in service, project, and research work. The intercorrelation between ratings by two supervisors was 0.66 and by two peers was 0.55. Since the criteria that he actually employed were based on ratings from two or more supervisors or from five or more peers combined, their reliabilities as estimated by the Spearman-Brown formula were above 0.80.

Processes

In his paper in this monograph J. P. Guilford stresses the importance of the question: What are the fundamental operations of thinking? This question, it seems to me, is a major present and future problem for research in creativity as well as in other areas of thinking. Data available thus far enable us to make only certain rather tentative statements in answer to this question.

Guilford notes that it is tempting to identify creative thinking with what, on the basis of his work employing factor analysis, is called divergent productive thinking. Included among the factors that fall under the heading of divergent productive thinking are those of adaptive flexibility, spontaneous flexibility, originality, and elaboration. However, Guilford goes on to point out that other factors that do not fall in that category, for example semantic redefinition and sensitivity to problems, also appear to be important in creative thinking. He concludes by suggesting that many other intellectual abilities may play a role in creativity.

Guilford takes the viewpoint that psychology demands more and more penetrating analysis if we are to understand thinking. He argues that not only more factor analysis but also other kinds of analyses are needed. I agree. Moreover, I am inclined to believe that one recent development in the psychology of thinking offers exceptional promise for increasing our understanding of the processes of thinking. This is the approach initiated by Allen Newell, J. C. Shaw, and Herbert A. Simon, which involves treating the thinker as an information-processing system (Newell *et al.*, 1958*a*).

Bruner, in his paper in this monograph, deals with this new approach at some length, expressing his own enthusiasm concerning its promise. For that reason I shall devote less space to it here than I had originally planned. I

refer the reader instead to Bruner's paper, to papers by Newell *et al.* (1958*a*, 1958*b*, 1958*c*, 1959*a*), and to a paper that I have published elsewhere (1960*a*) describing the approach in more detail and exploring its possible extension to the area of human motivation.

The most notable successes of the information-processing approach thus far have been in the study of human problem solving. In employing this approach, an initial attempt is made to identify the processes important in a particular kind of problem solving. Then an attempt is made to write in a precise language a program that employs these processes and that stimulates the thinking of the human individual solving such problems. Such programs are typically complex. Since they are written in a precise language, they may be run on a high-speed electronic computer to determine what behavior they produce. It should be emphasized that the purpose is not to make the computer think but simply to use the computer to determine whether the program carried out simulates adequately the behavior of the human subject.

It should be apparent that the use of this particular approach in thinking necessarily focusses attention on process. To simulate a given kind of thinking, it must first be possible to identify the important processes involved. Space does not permit here a detailed discussion of processes in thinking, but one distinction is essential for present purposes. This is the distinction between algorithmic and heuristic processes in thinking. An algorithm is a process for solving a problem that if followed guarantees that the problem will be solved in a finite number of steps if the problem has a solution. If, for example, we wish to find the maximum for a function and are familiar with the methods of the calculus, we take the first derivative, set it equal to zero, solve for x, and then continue with one of several alternative procedures. If we wish to find the square root of a number, we may use the simple algorithm that we learned in high school if we can remember it.

A heuristic is a procedure for solving a problem that if followed may aid in solution but offers no guarantee of doing so. In his book *How To Solve It*, the eminent mathematician Polya (1945) has dealt at length with the use of heuristics in problem solving at the level of high school mathematics. One heuristic that he describes is "working backwards." A beginning is made with the result desired and the attempt is made to work backwards step by step toward the given result. Another heuristic is to think of another problem similar to the one being solved, one with a known solution, and then to use the procedure successful in solving the similar problem in attacking the present one. Still another heuristic, and one familiar to all of us, is the use of analogy. Often in a problem-solving situation we try to think of an analogy to some other situation, one that will suggest an attack on the present one. In his two-volume work *Mathematics and Plausible Reasoning* Polya (1954) deals at greater length with the use of heuristic procedures in mathematical problem solving.

The importance of heuristic procedures in human problem solving has until recently not been well understood. For those problems for which simple algorithms are known, such procedures are of course preferred. They have the important advantage that they guarantee solution to the problems if the problems have solutions. For many important classes of problems, however, no algorithms are known. For some types of problems for which algorithms are

known the time required to carry out such procedures is prohibitive. Chess, for example, is a finite game. One possible algorithm for playing chess would be to consider all possible continuations of the game from the existing position to termination and then select the one that would lead to checkmate of the opposing king. If this procedure were employed, it is unlikely that a single chess game could be completed in a lifetime even if the players could work at the speed of the fastest electronic computers (Newell *et al.*, 1958*b*). Instead, chess is played by using such heuristics as "protect your king" and "try to control the center of the board."

Within the past few years programs have been written employing heuristic procedures and simulating important kinds of human thinking. One example of such a program is the Logic Theorist, a program written by Newell *et al.* (1958*a*) capable of discovering proofs for theorems in elementary symbolic logic. A variety of experiments have been carried out in which this program has been run upon the Johnniac, a high-speed digital computer. The effectiveness of the program is impressive. *Principia Mathematica* by Alfred North Whitehead and Bertrand Russell is, of course, the "classic" of modern symbolic logic. In one experiment the Logic Theorist, employing the same axioms, definitions, and rules used in the *Principia*, was presented with the problem of constructing in sequence a valid proof of the first 52 theorems in Chapter 2 of the *Principia* in the order in which they appear there. Whenever a theorem was proved, it was stored in memory and was available for use together with the original axioms in proving subsequent theorems. Under these conditions, the Logic Theorist succeeded in proving 38 of the 52 theorems, the time required to construct a proof varying from less than 1 min. to more than 15 min. It should be emphasized that the Logic Theorist does not carry out exhaustive searches of all possibilities. Rather, like human thinkers, it uses heuristic methods in conducting selective searches leading to the discovery of proofs for theorems.

Programs have also been written in simulation of playing chess (Newell *et al.* 1958*b*), discovering proofs in plane geometry (Gelernter and Rochester, 1958), binary choice (Newell and Simon, 1959), recognizing patterns and rote learning (Newell and Simon, 1959), and intelligent learning (Newell *et al.*, 1959*b*).

At this point, the question may be raised as to whether it is possible to simulate the processes involved in various kinds of creative thinking. Newell *et al.* (1958*c*) have dealt with this question at some length. It may be emphasized in this context that the discovery of proofs for theorems is a type of thinking that, when carried out by Whitehead and Russell, was regarded as highly creative. It is interesting to note that this program, when run on the computer, actually produced a proof for a theorem that was more elegant than that originally discovered by Whitehead and Russell (Newell *et al.*, 1958*c*). Programs are presently in use in industry for designing electrical motors, a task that when carried out by engineers is regarded as involving at least some degree of creativity. The consumer's requirements when received are punched on cards and fed to the computer together with the program. The output of the computer is a set of design specifications that are actually employed in manufacturing the requested equipment. Programs are also now available that compose music (Hiller, 1959). A program has been written at the University of

Illinois, Urbana, Ill., employing the entire set of 14 rules of first-species counterpoint. When run on the Illiac computer, it has produced music that is strongly reminiscent of Giovanni Palestrina. Other programs, in contrast, have produced music more like that of a Béla Bartok string quartet.

The conclusion that I believe is justified is that it is possible to simulate at least many of the processes important in thinking that results in the production of novel and worthwhile products, that is, in creative thinking. Simon and Newell (1958) have predicted that within 10 years a computer will discover and prove an important mathematical theorem and compose music regarded as aesthetically significant.

We may return at this point to the question of whether the processes of creative thinking are the same as or different from those involved in problem solving, decision making, or other kinds of thinking. The evidence available thus far suggests to me that the answer is "partly the same, partly different." It appears probable that many of the heuristics now being identified as important in problem solving will also turn out to play an important part in creative thinking. I think it unlikely that we shall ever be able to differentiate sharply between creative and noncreative processes: between those processes that occur in thinking resulting in novel and worthwhile ideas and those that occur in thinking that fails to produce such results. Thinking, I believe, is essentially unitary. Although it is useful to distinguish among kinds of thinking, such as problem solving, decision making, or creativity, the processes involved in one kind also appear often to be important in other kinds. On the other hand, it also appears probable that certain kinds of processes are much more important in one kind of thinking than in another. For example, metaphor as a heuristic seems likely to be much more useful in creative thinking, particularly in certain areas, than in problem solving or decision making.

Disagreement exists concerning the question as to whether the processes in creativity in one field are essentially the same as or different from those in other fields. Is creativity in science, for example, essentially the same as that in the creative arts? Again, it seems highly probable that the answer is "partly the same, partly different." Certain heuristics, for example, the use of analogy, appear to be of such general applicability that they find use in many different areas. Other processes, however, including particularly the algorithms, appear limited in their applicability to a narrower range of endeavor. In sum, it seems probable that some of the processes important in creative thinking in one area will also turn out to be important in a number of other areas. Some are likely to be useful only within a much narrower field of creative work.

It is of importance to note that if a factor analytic, rather than an information-processing approach is taken to these same questions, essentially the same answer is, I believe, obtained. The results described by Guilford in this monograph indicate that many of the factors important in creative thinking are also important in problem solving and other kinds of thinking. Some of the factors, however, appear to be much more important in creative thinking than in other areas. Similarly it appears that factors important in creative thinking in science in part are the same and in part are different from those important in other areas of creative activity.

A major problem for the future is the further study of the processes impor-

tant not only in creativity but in all kinds of thinking. Encouraging progress has been made, particularly within the past five or ten years, but much more work is needed.

Tasks

Very little systematic research has been devoted to the exploration of the relation between creativity and the characteristics of the task upon which the individual undertakes to work. The conviction exists in a number of research laboratories that the choice of the general task on which the individual is to work is an important factor in determining whether or not he will be creative. Indeed, in at least one large laboratory there is an official policy that, if an individual fails to be creative within two or three years, he is reassigned after consultation with him to a different area; if after two or three years more he is still not producing any new or worthwhile ideas, he is reassigned to still a different area. This policy is based on the viewpoint growing out of experience that an individual uncreative in one area may often show considerable creativity in another. That characteristics of the tasks chosen for attack are important factors in creativity seems quite probable. However, systematic research data bearing on this question are largely unavailable.

The selection of tasks for use in experimental studies of creativity is at present difficult. Little progress has been made in devising and securing wide use of standard tasks for eliciting creative thinking, so that comparisons of the effects of variables can be made across studies. Each investigator instead has tended to prepare and employ his own tasks or to modify in significant ways those that others have employed. Moreover, little is known of the dimensions in terms of which such tasks can best be characterized.

Among the dimensions that might be used to describe tasks and that appear to deserve further study are the following: (1) the extent to which the task involves convergent or divergent thinking (Guilford, in this monograph); (2) the extent to which the individual working on the problem gets feedback during the course of work and can utilize the feedback; (3) the extent to which the task can or must be completed in steps; (4) the class of heuristics appropriate to the problem; and (5) the type of criteria employed to determine whether the task is completed, for example, whether what is produced is "satisfying" or "correct."

Disagreement exists as to the level of complexity desirable in tasks employed in experimental studies. Some employ single tasks requiring as much as two or three hours and even more. Others express a strong preference for much simpler problems, arguing that the use of complicated tasks involves the danger of loss of experimental control and pointing out that, when complex tasks are used in assessment of achievement, relatively low reliabilities are often obtained. My own viewpoint is that there is no single answer to the question of how complex a task should be for use in experimental work. The desirable complexity of the task will vary markedly, depending upon the purpose of the particular research. If the interest is in investigating distribution of effort, strategies of search, or the effect of the separation of evaluation from production of ideas, then it often may be found desirable to employ relatively complex tasks. For other purposes, for example the use of factor analytic techniques, the use of very simple items may be necessary in order to get sufficiently reliable measures

in a large number of areas. In short, the question of optimal level of complexity of tasks would seem to depend in large measure upon what is being investigated.

Persons

Perhaps not surprisingly, more attention thus far has been given to the study of persons than to any of the other five areas. Most studies in this area have been concerned with determining the relation between various characteristics of persons and creativity. Two general methods have been employed. One involves obtaining a measure of the creativity of each of a fairly large number of individuals and then correlating with this measure other traits or abilities. The measure of creativity may involve simply some count of the number of creative products that each individual has produced, but more often it involves judgments or ratings as discussed above. In some instances, a score based on performance on a number of tests thought to measure creativity has been used as a criterion.

A second general method employed is that of identifying by some procedure a group of individuals who are clearly highly creative in a given area. The characteristics of individuals in this highly creative group are then compared with that of some appropriate less creative group to determine what characteristics distinguish the more from the less creative.

A number of interesting findings have emerged from the work completed to date. Some of these will be described here to illustrate the nature of the progress being made in this field.

One question concerning which data are available from several studies is that involving the nature of the relation between performance on intelligence tests and creativity. The results may be summarized in two statements, both of which are important: (1) Within a highly creative group, there is ordinarily little or no correlation between performance on tests of intelligence and degree of creativity. (2) The average intelligence of the members of a highly creative group is nearly always quite high; often most of the members in such a group rank in the upper 1 per cent of the population in performance on intelligence tests.

Thus, for example in the study of about 100 engineers in a research and development laboratory (Taylor, 1958), I obtained a correlation of only 0.20 between scores on the Concept Mastery Test and ratings of creativity and a correlation of only 0.07 between performance on the same test and ratings of originality obtained six months later. However, the average performance of the 100 men was quite high on this intelligence test developed by Terman (1954) for use in his studies of gifted adults.

The most extensive program of research now in progress concerned with the identification of individual characteristics related to creativity is that being carried out at the Institute of Personality Assessment and Research at the University of California at Berkeley, Calif. Among those participating in this program are Donald MacKinnon, Richard Crutchfield, Harrison Gough, Frank Barron, and Ravenna Helson. The general procedure that they have employed is to bring to the institute for several days a highly creative group of individuals and then to use a large variety of tests and other procedures to assess the apti-

tudes, abilities, interests, and traits of members of the group. Groups studied thus far have included 20 writers, 40 architects, 45 research scientists, and 41 mathematicians (MacKinnon, 1960). Here again the correlation between intelligence and creativity among the members of a group has been found to be low. Thus for example the correlation between performance on the Concept Mastery Test and the degree of creativity of the 40 architects as rated by a panel of five architects was −0.08. However, the mean score of each of the four groups just mentioned was very high (MacKinnon, 1960).

In selecting the members of a highly creative group, the range of variation with respect to creativity is, of course, markedly restricted. Since it appears that highly creative people tend to be highly intelligent (but only a fraction of highly intelligent people are highly creative), selection of the members of such a group also restricts the range of variation with respect to intelligence. Hence the fact that little or no correlation is obtained between intelligence and creativity within such a group may be accounted for in terms of the well-known effect of restriction of range upon the correlation coefficient. The statement that creativity shows little correlation with intelligence, taken alone, is quite misleading.

Although strong convictions concerning the relation of college grades and later creativity are encountered often, very little evidence is thus far available. In the study of engineers (1958) to which I previously referred, correlations were obtained varying from 0.26 to 0.35 between grades for the last two years of college and ratings of creativity. In a study of a group of 70 physicists in another laboratory, I obtained correlations varying from 0.23 to 0.37 between grades for the same period and rated originality. It is interesting to note that in the same group I obtained correlations varying from 0.34 to 0.55 between such grades and ratings of quantity of work. In neither study was there any evidence of curvilinearity in the relation between grades and creativity; the higher the average grade level, the higher were the ratings of creativity.

Of particular interest in the results of the work at the institute is the fact that all of the highly creative groups studied thus far have been found to share certain characteristics that distinguish them from less creative groups. This is true, for example, with respect to certain characteristics of their interest patterns as measured by the Strong Vocational Interest Blank. Those high on creativity tend to score high on such scales as psychologist, architect, author-journalist, and specialization level, and to score low on such scales as purchasing agent, office man, banker, carpenter, and policeman. These findings suggest that creative people are less interested in small detail, in the practical and the concrete, and more interested in the meanings, implications, and symbolic equivalents of things and ideas (MacKinnon, 1960).

The Study of Values is a test devised by Allport *et al.* (1951) to measure the degree to which an individual holds each of the six basic values originally described by Spranger: theoretical, economic, esthetic, social, political, and religious. Data obtained at the institute show that members of the highly creative groups studied tend to score relatively high on both theoretical and esthetic values, regardless of whether the members include research scientists, architects, mathematicians, or creative writers.

The performance of these several groups on the Minnesota Multiphasic Per-

sonality Inventory was of interest in two respects. First, all of the highly creative male groups tended to score high on the femininity scale. This does not mean that these men were effeminate in manner or appearance. Rather, it indicates that they share certain characteristics more often associated with women than with men in our culture, including an openness to their own feelings and emotions, a sensitive awareness of self and others, and esthetic interests. Second, members of the highly creative groups tended to score high on the eight clinical scales of the Minnesota Multiphasic Personality Inventory, their mean scores ranging from 5 to 10 points above the general population's standard score of 50. Although this would be suggestive of psychopathology in other persons, it is perhaps better interpreted here in terms of richness and complexity of personality and a general lack of defensiveness (MacKinnon, 1960).

The Myers-Briggs Type Indicator is a test recently developed (Myers, 1958) to assess individuals with respect to the personality types described by Carl G. Jung. The test is intended to determine an individual's preference for: a judging attitude versus a perceptive attitude toward life; an extraverted versus an introverted orientation to life; sensation versus intuition; and thinking versus feeling. The most striking finding was with respect to sensation versus intuition, where the former emphasizes becoming directly aware of things by way of the senses and the latter involves indirect perception of the deeper meanings and possibilities inherent in things and situations. Of the general population, 75 per cent show a preference on this test for sensation. In contrast, 100 per cent of the architects, 100 per cent of the mathematicians, 93 per cent of the research scientists, and 90 per cent of the writers in the groups studied showed a preference for intuition (MacKinnon, 1960).

Thus far we have given attention to the results from research at the institute that indicate ways in which their highly creative groups are like each other and different from the general population. Many findings are available from this and other research programs suggesting ways in which highly creative individuals in one area differ from highly creative individuals in other areas. Thus, for example, in performance of the Myers-Briggs test, whereas about 43 per cent of the general male popluation show preference for feeling over thinking, 65 per cent of the writers and 50 per cent of the architects and, in contrast, only 24 per cent of the research scientists showed such preference. To describe ways in which creative groups differ would take much more time than has been necessary to indicate ways in which they are alike; for that reason I am almost completely omitting reference to such findings here.

My purpose in this section has been to illustrate findings from research focussing upon persons, and for that purpose I have chosen largely results from work at the Institute of Personality Assessment and Research. Much other work has been done that I cannot even begin to describe. However, I must at least call attention to the series of studies by Roe (1951*a*, 1951*b*, 1952, 1953) of eminent physicists, biologists, anthropologists, and psychologists. To illustrate differences among creative groups, I might well have chosen Roe's finding (1951*c*) that biologists and experimental physicists tend strongly toward dependence upon visual imagery in their work, whereas theoretical physicists and psychologists tend toward dependence upon verbalization in their thinking.

It should also be noted that even my summary of results of recent work at

the institute is incomplete. For example, I have given no attention to Crutchfield's data (1958; see also Ludington, 1958) indicating that conformity is negatively related to creativity. Nor have I mentioned Barron's studies (1955, 1957, 1958) of the relation between personality and originality.

Much work has been done concerned with the identification of characteristics of the individual that are related to creativity. Much more work, however, must be done before any integrated set of general conclusions can be reached with confidence concerning the relation of attitudes, abilities, motives, and personality traits to creativity.

Environmental Variables

Only a limited amount of systematic research has to date been carried out investigating the effects of environmental variables upon creative thinking. That such variables are important seems probable. However, data to support this view are largely lacking. Badly needed are more studies of the effects upon creativity of: the nature of the immediate situation in which the individual works; the use of educational or other procedures intended to facilitate creativity; the amount and nature of the individual's interaction with others; the organizational setting; and both the subculture and the more general culture in which the individual lives, including particularly such factors as role expectations and values.

A variety of procedures are in use in industry and elsewhere intended to foster creative thinking, the best known among them perhaps being brain-storming. Nevertheless, experimental research is not yet available that would enable us adequately to evaluate the effectiveness of these procedures. My assistants and I (Taylor *et al.*, 1958) have published one experiment that in so far as its results can be generalized, leads to the conclusion that group participation when using brain-storming inhibits the production of ideas. Three studies have been published (Meadow and Parnes, 1959; Meadow *et al.* 1959; Parnes and Meadow, 1959) indicating that instruction in or training in the rules of brain-storming tends to increase the number of good ideas produced by individuals, at least for certain kinds of problems.

In an unpublished study that I made some years ago, I obtained data suggesting that one of the factors important in influencing the creativity of men in organized research laboratories is the nature of the relation that exists between the working scientist and his immediate supervisor, or more precisely, the supervisor who determines the atmosphere in which he works. However, although this appears plausible, further work is badly needed in exploring the importance of this relation and also of the relations of the working scientist to others in the laboratory.

In this context, attention should be called to the study of the performance of scientists in the laboratories of the National Institutes of Health, Bethesda, Md.; the study was carried out by members of the staff of the Institute for Social Research of the University of Michigan, Ann Arbor, Mich. (Davis, *et al.*, 1954). One interesting finding, for example, concerned the relation between group identification and scientific performance (Pelz, 1956): "If the scientists had a strong sense of belonging to the immediate group and the chief of that group was a relatively competent person, then the subordinates' performance was

high. But if the scientists had high identification with the immediate group and the chief was a relatively mediocre man, their performance was low."

On the basis of his studies of industrial research chemists, Stein (1956) has emphasized the importance of the differences among the roles that the same individual is expected to fill. Thus what is expected of the chemist in his role as a scientist differs from what is expected of him in his professional role, his employee role, or his social role.

That general cultural factors are important in their impact upon creativity seems apparent, but adequate data are lacking. It is paradoxical that, whereas the highly creative male groups studied at the University of California tended to score high on femininity, it is clear that many more men than women are outstanding in creative thinking. A number of studies by assistants in my own research program have demonstrated that men are significantly superior to women in solving a variety of problems, even when the two sex groups have been matched for intelligence, various special aptitudes, or specific relevant knowledge (Sweeney, 1953; Carey, 1958; Nakamura, 1958; Milton, 1957). Although the possibility of a biological factor here cannot be excluded, I am at present inclined to believe that these differences between the sexes are to be accounted for in terms of differences between the experiences of men and of women in our culture.

Data are available that suggest that cultural factors associated with socioeconomic level may also have impact upon creativity. Several studies have shown that those choosing science as a career tend to come disproportionately from middle class families (for example, Knapp and Goodrich, 1952). In the study of engineers referred to earlier (1958), I found a relation significant at the .01 level between family income during childhood and rated creativity many years later; those rated most creative tended to come from families with incomes around $3000, a figure that for the period of their childhood would be described as middle income. Other data suggest the possible relation to high-level performance in the sciences and the humanities of both geographical and religious factors. Thus the proportion of college graduates achieving recognition in these areas has been shown to vary with both the geographical location and church affiliation of the colleges attended (Knapp and Goodrich, 1952; Knapp and Greenbaum, 1953).

Perhaps because I was once a student of Boring (1950), I cannot bring this overview to a close without alluding briefly to the *Zeitgeist*. History—particularly the history of science—provides evidence of the importance of its influence upon the production and acceptance of ideas. Consider, for example, the relatively large number of independent simultaneous scientific discoveries that have occurred. Consider the number and variety of the contributions of ancient Greece during a period of less than 200 years and involving a population of the order of 100,000. Contrast that period with the Middle Ages. Compare it with the Renaissance. That the *Zeitgeist* must be considered in any attempt to provide a complete account of the conditions influencing creativity is apparent.

Conclusion

Research on creativity is a complex undertaking. Not only may it focus upon products, processes, tasks, persons, or environmental variables, but there

is also the additional complication that we have not yet mentioned: that of interaction between variables both within and among the five areas. Those investigating the relations between personality and creativity face the possibility that the relation between a particular trait and creativity may be highly dependent upon other traits of the individual. Similarly, data previously mentioned indicate that the relation between a trait and creativity will vary with the type of task or the field in which the individual chooses to work. The existence of such interactions makes much more difficult the task of the investigator, but that they do exist and are important is hardly to be doubted.

When one considers what has been accomplished thus far in the light of the complexity of the research task, one is impressed with how little knowledge of creativity we have as yet that is adequately based on research that is not impressive. On the other hand, if it is recognized that a large part of the systematic research in this area has been carried out within the past ten years and that the rate at which such research is being done appears to be steadily increasing, we may look forward to real progress in our understanding of creative thinking.

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Discussion of the Paper

QUESTION: I think of creativity in terms of a kaleidoscope. The more pieces of glass in a kaleidoscope, the greater the number of designs obtainable. I would expect that the better the memory of an individual, the more likely he is to be creative. Would you agree?

TAYLOR: An information processing approach to thinking would lead one to expect that, *ceteris paribus*, memory would be related to creativity.

However, at this point I would like to recall a point that J. S. Bruner made in his paper in this monograph. He stressed the importance for thinking of the way in which information is stored in the memory. How information is stored determines how accessible it is. Accessibility in particular situations of relevant information is probably much more important than the sheer amount remembered in terms of influence upon creativity.

QUESTION: Our research at the AC Sparkplug Division of General Motors Corporation, Flint, Mich. has yielded three different findings upon which I would like your comments.

First, we have found that persons who score highly on our own creativity tests are highly intelligent, but intelligent people do not necessarily achieve a high score.

Second, the average scores of high school graduates upon our test of creativity appear to be higher than those of college graduates upon the same test. Nevertheless, those who have been tested about five years after graduation from college and who have worked subsequently in engineering and research, again score higher.

Third, we offer a course intended to increase creativity. The training largely involves changing attitudes, removing the blocks to presenting ideas. In one study, two groups of hourly rated people took this course. The members of one group were selected because during the previous year the dollar value of the suggestions they had contributed under our regular suggestion plan was high. The other group was selected because the value of the suggestions they had made under the same plan was low. In the year after training, a 150 per cent increase was found in the dollar value of suggestions for the former group and a 110 per cent increase for the latter group.

TAYLOR: Your finding concerning the relation between measures of intelligence and performance on your test of creativity seems to me quite consistent with what I was trying to say concerning the relation between intelligence and creativity. Members of a group selected as outstanding in creativity will tend to be highly intelligent. Within that group there may be little or no correlation between intelligence and creativity. If one examined a random sample of the general population, one would expect a much higher correlation between intelligence and creativity. Nevertheless the correlation would not be perfect, and it would still be true that some individuals who were outstanding on intelligence tests would not, by other criteria, be judged as creative.

I have two comments with respect to your finding relative to the performance on your test of creativity of high school graduates, college graduates, and college graduates tested five years later. First, I wonder whether the groups tested at the three different levels are really comparable with respect to other characteristics. For example, I suspect that those tested five years after graduation and, who subsequently had been engaged in engineering and research were a more select group with respect to intelligence than the group of college graduates with which they were compared. On the other hand, it might be equally well suspected that the high school graduates were a less select group than the college graduates with respect to intelligence.

Second, it may well be that some parts of college education actually do make individuals less creative. I know of no adequate data bearing on this point, but I should not be too surprised if such data were eventually obtained. Some years ago I asked the director of a chemical research laboratory: "What about the Ph.D. training that men in chemistry receive; is it particularly important in facilitating creative work in chemical research?" He laughed and replied: "Well, it is my experience that it takes them about three years to get over the effects of their doctoral training. Now do not misunderstand me. Chemistry is a very complex field and I must have men educated to the doctorate. I can not prove it, but it is my impression that, in the course of getting the doctorate, men become so critical, learn to place so much emphasis on being right, learn to have so little confidence in their own ideas, it takes them about three years to recover from the effects of that training." I sometimes think that perhaps even graduate seminars in psychology may have similar effects.

That changing attitudes may increase creativity, as suggested by your third finding, is I think an important point. In a study carried out by one of my assistants data were obtained showing that group discussion procedures designed to improve attitude resulted in a significant increase in achievement in problem solving. More data are needed, but my own belief is that differences in attitudes are important in accounting for the sex differences both in problem-solving and in creativity to which I alluded in my paper.

J. P. Guilford (*University of Southern California, Los Angeles, Calif.*): It is evident that Taylor is agreeing with me on many points and I can say after listening to him that I agree with him a very large part of the time.

I would like to make two comments in relation to the finding at the University of California, described by Taylor, with respect to scores of highly creative groups upon *The Study of Values*, the test devised by Allport, Vernon, and Lindzey. You will recall that members of all of the highly creative groups studied tended to score high on both theoretical and esthetic values.

Scores on theoretical values are ordinarily found to be negatively correlated with those on esthetic values. This negative correlation is, however, an artifact resulting from the method that the manual provides for scoring the test. Were a different kind of item employed from forced-choice items, there might actually be a positive correlation between theoretical and esthetic values; at least, a negative correlation should not be expected.

The fact that all of the highly creative groups studied at the Institute for Personality Assessment and Research scored high on theoretical and esthetic values and not on economic or political values may have resulted from the particular kinds of creative groups that the Institute chose to study. Had they been dealing with people more concerned with behavioral content—that is with statesmen, politicians, salesmen, and other people who have to invent ways of dealing with people—I think they might have found high scores on other than theoretical and esthetic values.

Question: I am inclined to think that the processes involved in creativity, or the characteristics related to it, may vary with the level of performance or the scale of the task involved. If we ignore this probability, mistakes may be made in attempting to identify those who will be creative in a given area. Ig-

noring it would be analogous to the behavior of a coach trying to pick a team of marathon runners but who did not have time to try out all the candidates by having each of them run 25 miles. Instead, he had all of them run the 100-yard dash and excluded from further consideration for the marathon team all those who did not make the 100-yard dash in 10 seconds. Would you comment?

TAYLOR: Your point, I think, is well taken. It seems to me plausible that the processes important in one level of performance may often be quite different from those important in higher levels of performance. Similarly, the processes involved or the characteristics related to achievement in a short-term task may differ from those related to performance in a task requiring much more time.

Your point reminds me of another that is of some importance. Suppose that we succeed in identifying a number of characteristics that are significantly correlated among adults with creativity in a given area. The question still remains whether these same characteristics, when measured among children or among adolescents, will be predictive of creativity in adult life.

QUESTION: I think many of us here have been interested in the possibility of learning more about problem solving and creativity by using a computer as a tool. Would it be possible for you to sketch briefly the procedure that might be used in trying to develop a computer simulation of a high school sophomore discovering a proof in plane geometry?

TAYLOR: Let me give a brief answer to a complex question. First, it should be emphasized that the computer does not "do your psychology for you." What the computer does is to enable you to determine rapidly and accurately what behavior your simulation or model will produce once you have constructed it.

In undertaking to simulate the behavior of the high school sophomore, you must first identify in detail and as precisely as you can the kinds of processes that the sophomore seems to be using in accomplishing whatever it is that he is accomplishing. The next step is the construction of a flow diagram. The flow diagram may be thought of, if you like, as consisting of a set of black boxes, each representing a particular operation or process. The purpose of the flow diagram is to represent the interrelation of the processes involved in the behavior you are trying to simulate.

The next, and a much more laborious task, is to translate the flow diagram into a program that can be run on a computer. To be run on a computer, a program must be written in a precise language. Since English, French, German, and others are not precise languages, you must use a specially constructed language. A variety of such special languages exist, their characteristics varying depending upon the purposes for which they are to be used. For simulating human behavior, the so-called information-processing languages are perhaps most appropriate, having been developed by Newell *et al.* (1958*a*) for this purpose.

Once you have written a program, you run it on a computer to determine the results it will produce. You then compare the behavior of the computer with that of the human subject to determine how accurate the simulation is.

You may then add to or modify your program to increase the adequacy of the simulation.

What simulation does is to provide a kind of test of your hypotheses that certain processes are important in a particular kind of behavior.

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ON SOME ASPECTS OF THE CREATIVITY PROBLEM IN THINKING

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This report describes an approach that may be useful in dealing with some aspects of the creativity problem in thinking. It also describes some exploratory research based on the approach and it offers suggestions for future research. The approach may be characterized as phenomenon-centered since it starts with a specific case of a particular phenomenon, tries to view it as unbiasedly as possible (deliberately suspending theoretical assumptions concerning the phenomenon), and attempts to be guided by characteristics of the phenomenon throughout the course of research. The approach may also be characterized as variational since it varies the conditions under which the phenomenon is studied and also turns to a variety of theories and disciplines to obtain suggestions for research and interpretations of results (for further delineation of the approach and its applications see Luchins, 1955, 1957, and also Luchins and Luchins, 1959).

Contemporary research on creativity is concerned mainly with studying creative individuals (Bellak, 1958; Drevdahl and Cattell, 1958). Research is also needed on a somewhat neglected aspect of the creativity problem, namely, how to characterize and study creative acts rather than actors, for example, how to investigate the act rather independently of the personality of the individual who performs the act. (An analogy may be found in traditional logic where an act of reasoning can be designated as valid independently of who did the reasoning.)

Concerning Characterization of Creativity

As a first step in the characterization of creativity, it seemed advisable to bracket our theoretical biases and look first at the way the term creativity is used in ordinary life situations. For several years, therefore, my colleagues and I have asked students to keep records of the contexts in which they and others use the terms creative and noncreative. We have also questioned adults and children concerning what they mean by the terms. To establish the discussion I refer to one such study done with 60 junior high school students and with 35 college students.

(1) Of the 95 subjects only one, a college student, failed to respond to the first question, "What is the meaning of creative?" Others gave more than one criterion or definition. The 35 college students gave 74 responses and the 60 children gave 81 responses to this question.

(2) That creativity involves producing or making some concrete object was reported more often by the children than by the adults (33 per cent of the children's responses compared to 7 per cent of the adults' responses). The children more frequently described creativity as involving something beautiful, useful, or socially valuable (25 per cent compared to 4 per cent for the adults' responses). On the other hand, more adults characterized creativity in terms

of originality, invention, and innovation (45 per cent compared to 18 per cent for the children's responses).

The college students used such terms as new, original, different, imaginative, ingenious, and unique. Some indicated that it had only to be new for the particular performer, while others stipulated that it had to be new in a given area, and still others failed to specify what they meant by new or original.

(3) Some referred to creative as self-expression or expression of one's emotions, desires, or feelings (8 per cent of the adults' and 3 per cent of the children's responses). About 6 per cent defined creative as the ability to think deeply or to express thoughts well or (one child's definition) as "the ability to communicate." About 6 per cent referred specifically to the arts and sciences. However, when the subjects were asked to give examples of creative people, famous artists and scientists predominated (71 per cent of the 63 examples contributed by the adults and 65 per cent of the 34 examples contributed by the children). While every adult gave at least one example of what he regarded as a creative person, almost one half of the children failed to do so.

(4) When asked, "What is meant by noncreative?" the modal response category for adults (44 per cent of their 71 responses) referred to repetition of one's own or others activities, using such terms as following, copying, imitating, conforming, habituation, stereotypy, lack of change, or lack of originality. Such answers constituted 14 per cent of the children's 57 responses. The children's responses also referred to noncreativity as the inability to make or do something good, beautiful, useful, constructive (28 per cent), or simply as the inability to produce or create an object (17 per cent), or even equated noncreative with destructiveness (11 per cent).

(5) The question "Can there be thinking that is not creative?" was answered affirmatively by over 80 per cent of each group. As examples of noncreative thinking, adults listed mechanical or rote repetition of others' ideas, unreflective thinking, recall of the past, wishful thinking that remained unfulfilled as in daydreaming and reveries, and thinking about destructive acts (for example, contemplation of suicide or murder). Eight per cent of the adults asserted that the thinking done by the average person was noncreative, but 14 per cent claimed that all thinking, including daydreaming, was creative. Five per cent said that thinking was always creative for the thinker but might be noncreative for others. As examples of noncreative thinking, the children listed thinking about diversions or recreation (for example, sports, candy, gambling, and members of the opposite sex), having thoughts that were foolish, stupid, useless, or that were mean, profane, abusive, or concerned immoral, destructive, or antisocial acts, thinking that was automatic, habitual, or unreflective, reminiscing or trying to remember events, having thoughts based on something one had read or heard, copying others' ideas, and inventing something that had already been invented. When asked to mention specific noncreative activities, both adults' and children's modal category contained activities described as routine, mechanized, or habituated activities that called for little or no problem solving or decision making.

Analysis of our subjects' responses show that their definitions of creative almost run the gamut of the various definitions found in the scientific literature.

However, no subject explicitly stated, and very few implied, that everything one does is creative, as long as it involves an element of change from what one did before, no matter how slight, trivial, or inconsequential the change. However, precisely such a definition of creativity is explicit or implicit in some scientific writings (Leuba, 1958; Barnett's broad conception of innovation, 1953). A similar meaning is implied in the manner in which creativity is studied by some investigators, for example, any change from a habituated activity, such as writing a letter of the alphabet backwards, is regarded as measuring creativity (cf. Cattell and Tiner, 1949). Some definitions imply that any expressive act or any artifact in nature, material or immaterial, is creative. Indeed, some definitions are so broad that to be creative is tantamount to doing or to being. This makes the concept of creative redundant. However, in life the distinction is made between creative activities and other activities. Certainly the subjects made a distinction. Is the uniqueness and richness of creative activities approached by labeling as creative any arbitrary concatenation of events or by measuring creativity in terms of any change of a habit? It is possible, of course, to define creativity freely, but then the definition may have little relevance for life phenomena that are ordinarily called creative. One aspect of the creativity problem is how to characterize creativity so that the scientific conceptualization penetrates and reflects phenomena that are ordinarily called creative and differentiates them from phenomena that are noncreative.

Many of the subjects (45 per cent of the adults and 18 per cent of the children) named originality, invention, and ingenuity as features of creativity. Beauty, usefulness, and social worth and constructiveness were features ascribed to it by some subjects (25 per cent of the children and 4 per cent of the adults). However such features are conspicuously missing from some definitions of creativity in the literature. In fact, some writers consider the omission to be a virtue (Leuba, 1958). Nevertheless it seems that the characterization of creativity, for purpose of scientific study, should include parameters that refer to intrinsic properties of the act and that refer to such concepts as originality and ingenuity and, perhaps, even beauty, usefulness, and constructiveness. The domain of variation of the parameters can be sufficiently broad that the parameters, under specific circumstances, may assume zero or negative values. In this way, the characterizations can encompass creative acts, or evaluations of such acts, that do not involve a property in question (zero value of the corresponding parameter) or that, for example, are considered ugly or destructive (negative values of the corresponding parameters). However, if the characterization does not involve such parameters, how can it encompass creative acts in life, or evaluations of such acts, that do involve the corresponding properties? Analogously, if thinking is considered capable of being "structured" and "sensible" (cf. Wertheimer, 1945), then a characterization of thinking that includes the parameters of "structure" and "sensibleness" or "fittingness" can be applied to thinking that is arbitrary and rather "senseless" by allowing these parameters to tend toward zero. Nevertheless a characterization that excludes these parameters cannot encompass, or loses essential features, of thinking that is more structured and sensible.

It may be contended that the parameters refer to features that are similar

to the mystical ideals of the true, the beautiful, and the good. They are values, and values have no ontic existence, no place in the world of facts, and are not the subject matter of science. They are epiphenomena, as is the wetness of water.

These contentions may be encountered by noting that so-called epiphenomenal aspects of matter are studied in chemistry by rheologists. Moreover, at least some psychologists (Wertheimer, 1934, 1935; Köhler, 1938) have recognized that values have a place in the world of facts and may be studied scientifically.

It is sometimes argued that value-laden conceptions of creativity depend on social norms and may differ for different cultures and for different people in the same culture. Without denying this, I stress the importance of paying attention to the so-called objective evidence used by different persons in their evaluations of various acts or objects as creative. This may help to determine whether the evaluations have some kind of intrinsic relationship to the objective evidence employed and to the act or object evaluated. Such a procedure seems more justified than to assume from the onset that the relationship is necessarily extrinsic, arbitrary, personal, and subjective.

There is a need for research, preferably of an interdisciplinary nature, to investigate what is considered creative by peoples of different cultures and by various kinds of people in the same culture. The accumulated knowledge could then be analyzed to discover the roles played by social conventions, personality factors, and more intrinsic factors in the activity evaluated. This may lead to the discovery of the variables involved in creativity and also indicate certain invariants common to some or all of the conceptions of creativity. The invariants may be in the form of relationships that assume different concrete forms in different specific contexts. In short, the invariants, analogous to fixed points in mathematical transformations, would be the properties (or relationships or principles) that stay fixed even when transformations or changes are introduced in the evaluators or in the setting under which the evaluations are made.

Variations of Experimental Conditions

The subjects frequently described noncreative as involving imitation and habituation. These phenomena alone have been studied intensively by us for over two decades via a phenomenon-centered variational approach. These suggest that habituation and imitation or conformity tend to be maximized by a certain social atmosphere (which stresses speed and heightens emotional tension), but that they are not effectively minimized by a relaxed social atmosphere. However, when subjects had certain opportunities for decision making with regard to the tasks, they tended to show less mechanization and conformity and more original solutions of the problems. These results were found with tasks that lent themselves to rote learning and did not require much creativity. It is of interest now to begin with a task that requires considerable creativity and to study, first, what conditions tend to extremize creativity and, second, the effect of introducing the same conditions that tended to extremize mechanization and conformity in previously studied rote-learning experiments.

Analysis of experimental data and of subjects' responses to the above-men-

tioned questionnaire concerning creativity have led to the following tentative working hypotheses for our research.

Among the factors that distinguish creative thinking from thinking that is less creative are: a greater unity or wholeness of the task rather than extreme division of labor; a relatively high degree of freedom for action or decision making by the thinker; and a kind of discontinuity between the present and the past rather than merely repetitive or habitual use of past knowledge. (None of these factors by itself is sufficient to guarantee creativity.)

Although these factors are interrelated, research has been undertaken to vary one of the factors experimentally and to study the influence on other factors.

The wholeness of the task has been varied by beginning with a task that has a high degree of internal structurization (for example a mathematical problem) and: (1) allowing some subjects to survey the task as a whole and then work on it, individually or in small groups; (2) allowing other subjects to survey the task as a whole but with a subject or group assigned to work on only one part of the task; and (3) allowing no survey of the task as a whole before assigning one part of it to a subject or group.

Moreover, the division into parts or subtasks has been done along both prostructural and contrastructural lines. Further clues for research on the factor of wholeness of task may come from industry, where the effects of division of labor have been studied in recent years.

The degree of freedom afforded the subject for decision making has been varied through: (1) the extent to which the subject participates in the formulation or creation of the problem on which he subsequently works; (2) the structure of the problem in terms of the number of solutions it permits, for example, whether it has a unique solution, more than one solution, or no solution; (3) the admissibility conditions imposed on possible alternatives for a solution (and which may or may not be considered as part of the problem per se); and (4) the instructions and the social atmosphere in which the work is done.

Only exploratory research has been done on this factor. We need, for example, to investigate further the influence on creativity of varying admissibility conditions. Creativity sometimes consists of the ability to establish the possibility of a solution, under given admissibility conditions, no matter how restrictive they may be. On the other hand, creativity may consist of demonstrating that no solution is possible under given admissibility conditions, as when it was proved that the Greek construction problems of trisecting a general angle, doubling the volume of a cube, or squaring a circle, are not solvable under the classical admissibility conditions that limit construction tools to a straight edge and compass.

Creativity often is manifested in the ability to formulate or create problems or in the ability properly to reformulate problems. (Indeed, a solution to a problem may be regarded as a certain reformulation of it. In fact, one aspect of the creativity problem in thinking is just the problem of what constitutes a problem and a solution.) We need experimental investigation of creativity as manifested in the creation or formulation and reformulation of problems.

Another path for research, which has not yet been explored, concerns the influence of personality and psychopathology on the degree of freedom of the thinker, for example, obsessive compulsive and manic patients may be studied

to see how their perceptive and cognitive grasp of a problem and freedom for decision making seem to be affected by pathology.

Discontinuity with the past refers to activities that reveal deviation from the past and in which an individual does not merely automatically repeat habits or knowledge acquired in the past. Discontinuity thus refers to deviation from the individual's past and not necessarily to the past of his culture or the body of accumulated thought on a given subject. In other words, the term creative is not being reserved for acts that have historical priority. It seems that an act could manifest a certain degree of creativity without having to be compared with the whole body of accumulated knowledge in the area and without concern with problems of historical priority. The history of scientific thought shows that it is not unusual for several individuals to make similar discoveries independently of each other. The mathematician Wilder notes that when a mathematical concept "is about to make its appearance, it is most likely to do so through the medium of more than one creative mathematician" (1953, page 425). To claim that only the first discovery is creative is to ignore the likelihood that these acts may have genotypical similarities that may be the hallmarks of creativity. Moreover, it may intensify quarrels over the so-called invention of a scientific idea or artistic form. The mathematicians Courant and Robbins note that the quarrel over "priority in the 'invention' of the calculus set an unfortunate example for the over-emphasis on questions of precedence and claims to intellectual property that is apt to poison the atmosphere of natural scientific contact" (1941, page 399).

Moreover, it sometimes happens that individuals, even children, rediscover scientific and mathematical principles. Such rediscoveries may be judged as creative if historical priority is not considered essential.

Experimental investigations of the factor of discontinuity with the past have followed two avenues. As already indicated, habituated behavior has been investigated involving a low degree of discontinuity with the individual's immediate past (Luchins, 1942; Luchins and Luchins, 1959). Also relevant is exploratory research on methods of teaching aimed at helping children to discover or, rather, rediscover, certain mathematical principles. Results of these teaching methods are assessed in terms of features of the on-going learning process, for example, whether it is characterized by understanding or perceiving of structural features of the problem situation and also in terms of the quality and extent of transfer to other problem situations. Further examples and clues for experimentation are suggested by discussions of methods of teaching and learning in mathematics and physics in the writings of Stern (1949) and Wertheimer (1945). The kind of research this can lead to is exemplified in reports by Aram (1957) and by Luchins and Luchins (1947).

Study of Creative Acts in Retrospect and Prospect

A variational approach may be used to investigate creative acts in retrospect or prospect, that is, after the act is completed or before it is completed. A "pure case" study—intensive investigation of a few selected activities—seems more appropriate here than statistical trends based on surveys of many cases. Also, there has been generally an attempt to use what *Gestalt* psychologists call a method of analysis "from above" in determining the parts into which to divide

an activity for purposes of analysis (cf. Koffka, 1935; Köhler, 1929; Wertheimer, 1925).

For examples of creative acts that can be studied in retrospect, we turn to "pure mathematics" described as "the most original creation of the human spirit" (Whitehead, 1948, page 20) and, specifically, to problems in higher mathematics that were first solved in the last decade or two. Working together with mathematicians, I have selected a few of these problems and am interested in learning from the mathematicians involved, preferably both from those who finally solved one of the problems as well as from those who worked on it with only partial success, what they regarded as the obstacles to solutions and how the obstacles were surmounted. For purposes of comparison, a few open questions in higher mathematics ought to be studied to see what are regarded as the obstacles to solution and what modes of attack are being considered.

Prospective creative acts are provided by prospective graduate research. I am interested in having doctoral candidates, especially in the sciences and mathematics, keep records of the direction of their thoughts relative to the initiation and progress of their research. Another procedure is to assign a psychologist to do the record keeping. One difficulty is that of making students more adept at naturalistic observation, because they tend at the onset to be concerned mainly with efficient methods of categorizing and recording data in terms of a code.

Another area for investigation is the basis used for acceptance or rejection of research proposals made by candidates for advanced degrees or by applicants for research fellowships. Still another area is the basis used for acceptance or rejection of articles submitted to scientific journals whose purported purpose is to publish original contributions. Our own observations in these areas are replete with examples of emphasis on the formal design of the study, on rhetoric, and on popular or respectable topics or methods. This leads to the problem of how the design and expression of novel ideas influence their acceptance or rejection by society as well as to the broader problem of the communication and reception of novel ideas.

Communication and Reception of Creative Thinking

Creativity, although highly individual, occurs in a social medium and is a matter of social concern. Aspects of the creativity problem pertain to the invidual's ability to communicate his creative ideas and to actualize or bring into fruition activities implied in the ideas. A related aspect is that of the recognition of thinking as creative. History provides numerous examples of ideas initially ignored or ridiculed and subsequently acclaimed as creative. Many ideas now accepted were considered bizarre or visionary in the recent past, for example, an H-bomb or a flight to the moon. Sigmund Freud's concepts were initially received with the same ridicule and vituperation as Wilhelm Reich's ideas now receive in some circles. Many a paper lay unread for years in the office of a scientific academy because it involved ideas or symbols too difficult or unusual to follow. The disdain with which his contemporaries greeted Georg Cantor's innovations regarding infinite sets may have contributed to the mental condition for which he was institutionalized (Wilder, 1953). Perhaps

it requires a kind of creativity to apprehend that certain ideas are creative and are not necessarily absurd. To take another example from mathematics: attempts to prove the independence of the parallel postulate of Euclidean geometry led several mathematicians to assume a different postulate and to deduce results so contradictory to Euclidean geometry that they used them to rule out the possibility of a nonparallel postulate. It took the creativity of such mathematicians as K. F. Gauss, J. Boylai, and N. I. Lobachevsky, to recognize independently, that such results were not absurd but formed part of a non-Euclidean geometry that differed from the postulates of Euclidean geometry only with reference to the parallel line postulate (Courant and Robbins, 1941).

In this connection it is relevant to quote Oswald Veblen's comments in an address in 1952 to the International Congress of Mathematicians in Providence, R.I. "Mathematics is terribly individual. Any mathematical act, whether of creation or apprehension, takes place in the deepest recesses of the individual mind . . . . Mathematical thoughts must nevertheless be communicated to other individuals and assimilated into the body of general knowledge. Otherwise they can hardly be said to exist" (Veblen, 1952, pages 124 to 125). Citing Veblen's comments, Wilder concludes that "while in one of its aspects, mathematical creation is . . . terribly individual, in its other aspects it is not an individual affair at all" (1953, pages 424 to 425).

In short, in order for ideas to be recognized as creative by anyone but the creator, it is necessary that they be communicated. Communication and reception of ideas takes place in a social manifold that involves dimensions of space, time, and personalities. Whether they are considered creative seems to depend, to some extent, on how the ideas are communicated and received and on the point in the social time-space manifold at which the evaluation is made. We need to learn more concerning what there is about an idea, about the time and manner and circumstances in which it is communicated, and about the people involved in the communication, that contributes to the idea being ignored, discounted, or accepted and even actualized.

Some possible avenues of research are the following.

Investigation. Preferably of an interdisciplinary nature, it may be made of certain famous ideas: (1) that received almost immediate recognition and are still regarded as highly creative; (2) that received almost immediate recognition but whose creative quality did not stand the test of time and that are now not generally regarded as creative; and (3) that were initially ignored or were discounted but were later recognized and are still regarded as creative.

This study should concentrate not only on the nature of the ideas but also on the circumstances under which they arose and were accepted or rejected. Also of interest would be the roles different people played in these processes and why they played these roles. Such investigation may help to reveal the dynamics of acceptance and rejection of novel ideas. It may perhaps indicate, among other things, that novel ideas are accepted if they fit in with existing values or if they are accompanied by suitable changes in or additions to the existing values. Some light may be shed on the puzzling question of why certain individuals are not recognized as creative in the long run of history but are so regarded in the short run and vice versa. Perhaps the former had the ability to so transact with others as to make their ideas acceptable. E. E.

Morison (1950) gives an analysis of factors related to acceptance of continuous naval gun firing.

Research opportunities. A mental hospital provided research opportunities for study of the communication and reception of novel ideas under conditions easier to control and manipulate than the nonhospital community. Continued investigation of patients' novel ideas can be worthwhile, even if it only aids in a better distinction between a madman's scheme and a genius' dream or if it points to obstacles that make it difficult to distinguish between them.

This is not to imply that most patients in mental hospitals are creative, but certainly nonconformity is a feature of some creative people and also of many institutionalized patients.

While conducting and supervising group psychotherapy and action research (Luchins, 1959), I found that patients sometimes presented ideas that were ignored or even dismissed as bizarre. However some of these very ideas were later put into practice, when submitted to staff by someone in authority, occasionally to the confusion and chagrin of the patients who had originally advocated the ideas. Another example that patients' novel ideas may be ignored even when feasible is given by the patient who talked about making television movies, years before they were in existence, but whose ideas were regarded by the therapist as part of the patient's delusional system.

Clinical psychologists, by widening their conception of psychotherapy (Luchins, 1959), may contribute to the study of the communication and actualization of novel ideas. They can do so by assuming the role of mediator between the socially-inept patient and the social reality of the hospital and by engaging in action research aimed at realizing, in the hospital, some of the novel ideas suggested by patients. Because of the belief that such action research can be effective psychotherapy as well as fertile with hypotheses concerning the variables involved in social change, we have included it as an essential part of our clinical training program. Some of the preliminary work hints at the variables and dynamics of the communication of and resistance to acceptance and actualization of novel ideas. It is of interest to compare the variables and dynamics with those found under nonhospital conditions, for example, in the previously-mentioned study of the acceptance of graduate students' research ideas. Such comparisons may indicate differences under different field conditions as well as invariants or generalizations.

The factors that seem to affect the acceptance of patients' novel ideas include the following.

(1) The patient may be unwilling or unable to communicate his ideas, sometimes because he speaks in a private language, or more often, because other ideas and words become entangled with the main ideas as he rambles on, or because of the tone of his voice or distracting expressive behavior. What he says is rejected as just talk.

(2) The idea is considered infeasible because of a focus on possible danger to life, limb, or property that its actualization may entail.

(3) The idea is viewed in the context of the patient's personality dynamics and in terms of the patient's motives.

(4) The patient's reputation and status in the hospital are considered in deciding the value of his idea.

(5) There is concern with how actualization of the idea might affect existing rules, both written and unwritten. These rules may be unique to a particular ward or to a particular hospital service or prevail throughout the hospital.

(6) The idea is viewed as not in accordance with the formal and informal power structure of the hospital.

(7) It is viewed as not in accordance with the formal public policy of the non-hospital community, vested interests in the community (for example, professional and commercial organizations), or legal and governmental agencies.

(8) The idea may be viewed as a possible source of additional work or additional tension or as possibly upsetting routine ways of doing things.

(9) More generally, there may be sheer inertia that hinders novel ideas. There may be a desire not to become involved with worrying either about the "wheels within wheels" that may be set in motion if an attempt is made to introduce a new idea, or about the friction the turning of the wheels may produce.

It may not be out of place to mention here the need for research into conditions that may be varied, both in the hospital and outside of it, to make for more acceptance of deviation and for more tolerance of nonconformity in thinking. Here too is a place for the clinical psychologist to contribute to the study both of social change and psychopathology.

Laboratory types of experimental situations. These have also been used to study what happens when a novel idea is introduced to a group. My associates and I have recently begun research in which a group of subjects develop a group norm for the goal to be reached, for the method with which to reach the goal, or for both the goal and the method. A new individual, who has been set for a different goal and/or method, is then brought into the group. We have, for example, pitted method against method, method against goal, and goal against goal. We are interested in discovering the conditions under which the newcomer wins out and gets the group to accept his novel ideas, as well as in discovering the conditions under which the newcomer is ignored by the group. One of our graduate students, John Lozak, is now conducting experiments with such situations and it is hoped that he will report on them in the near future.

Examination of observations and experimental data. This suggests that some of the grounds that are used in rejecting novel ideas readily lend themselves to classification in terms of the material fallacies of logic, such as the fallacies of equivocation (for example, ambiguous and shifting terms) and the fallacies of unwarranted assumption (for example, begging the question, irrelevant conclusion, and irrational evidence as *argumentum ad hominem*). This suggests another research possibility. Give one group intensive training in understanding the nature of the material fallacies of logic and in appreciating why they should be avoided in their group discussions. Train another group to utilize some of these fallacies of logic in their discussions. This can be done, for example, by making it stylish in the group to judge an idea in terms of the motives or personality or ideology of the person who presents the ideas. These groups may be compared for the incidence of and reactions to novel ideas.

Some Social Influences on Creativity

Another aspect of the creativity problem concerns study of factors in our society that operate for or against creative thinking. Factors that may dis-

courage creativity are the growing mechanization of industry and the specialization and division of labor that have reduced into routine, repetitive tasks many activities that once called for ingenuity and skill. Many occupations offer little opportunity for the survey and solution of a whole problem and for the display of original thinking. Ironically, these occupational trends are accompanied by an emphasis (by guidance and vocational counselors) on the need for a person to actualize his self or his personality in his occupation. However, how can an individual actualize himself in positions that have little room for creativity? By becoming a robot? Moreover, the notion of actualization of the self in one's work involves an egocentric conception of work that itself requires evaluation. What is the effect on creativity of being concerned with profits to one's ego rather than the requirements of the task?

Despite the trend toward mechanization, specialization, and division of labor, there remain many positions in our society that need creative people if the goals for which the positions were established are to be met. This has led to interest in the development of measuring devices to determine and to select the potentially creative individuals for special education and training. With no intention of belittling this effort at measurement and selection of creative individuals, I raise the question of whether, if carried too far, it may result in further specialization in which there are specialists in society who do the creative thinking, as in A. Huxley's *Brave New World* (1946) and G. Orwell's *1984* (1949). We should not over look the possibility of developing creativity in people who do not display much of it. One step would be to examine ordinary teaching procedures to find out to what extent they contribute to or interfere with creative thinking. Our research on mechanization in problem solving indicates that emphasis on the response, rather than on the performance that led to the response, fosters an attitude favorable to mechanized behavior. This is clearly seen in so-called objective tests that, by their very nature, emphasize responses rather than the performance leading to the responses. Such tests seem to foster conformity with what a teacher or textbook said rather than expression of novel ideas. It is of interest to compare students' conceptualization of the subject matter and their preparation for the test when they are graded on objective tests marked by a machine as compared to when they are graded for a spark of original thinking (Luchins and Luchins, 1946). This is not difficult to do since there are courses (even in philosophy, psychology, and mathematics) that some instructors grade with objective tests while other instructors favor tests that allow for original thinking. Similarly, other educational procedures should be investigated for their influence on creativity, for example, the use of workbook exercises instead of class discussion or recitation.

I wonder to what extent creativity is hindered by the current emphasis on feelings and attitudes ("social process") rather than on content ("evidence"), on not doing things to create tensions in a group, on being a well-adjusted individual, on being understanding and reflective of others, and on behaving so that one is not unpredictable in his interpersonal relations. The emphasis is on social living and "togetherness" in the family, school, religious organizations, and mass media. Does this stress on interpersonal relations make "selling" of oneself or putting oneself across a co-requisite for putting one's ideas across? If so, what happens to creative individuals who are not adept at "selling" them-

selves? Moreover, if an individual has to concentrate on not creating tensions and on being accepting and reflecting of his fellow men, will he be able to devote his energies to creative thinking, and will he be likely to dare to deviate from others enough to have novel ideas? Some famous creative individuals recognized their need to remain apart from others for considerable periods of time (cf. Cantril and Bumstead, 1960, Frank, 1947). It is of interest to investigate whether the current emphasis on "togetherness" may create conditions that interfere with creativity. An exploratory study might compare the creative thinking of students and staff of schools that stress "social living" with those of schools that stress academic proficiency. There is also a need for controlled research to vary the emphasis on interpersonal relations in selected groups and to study what happens to the output of creative thinking.

Concluding Remarks

In conclusion, I shall point out that the teaching situation, even in undergraduate psychology courses, offers opportunities to engage with students in research activities described in the paper. I hope that more instructors and students will undertake such research. To do so effectively calls for lessening of the distinction between teaching and research and between learning and research. It requires an approach to teaching in which subject matter is presented not merely for memorization or verbal discussion but as a stimulus for research that is carried out by students and instructor as an integral part of the course. By making the teaching process a creative process we may learn more about creativity. Specifically, by focusing some of the research potential in the teaching situation onto the creativity problem in thinking we may learn about creativity as we engage in creative activities.

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IDEATIONAL REORGANIZATION OF IDEAS IN CREATIVE AND NONCREATIVE THINKING

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At the end of the 18th-century revolutions of thought that deposed the importance of reason, the intellectual world's attention, directed by David Hume, centered on man's capacity to recombine ideas. In this way man acquired knowledge and what he considered new was but a reorganization of the old. Human creativity or our creative imagination assumed an importance that would have been extremely surprising to Plato and Aristotle, who never thought in these terms, but who if forced, might attempt to explain what we call originality as a product of reason, intellect, or Nous.[5]

Psychology, still a branch of philosophy for most of the century following, was largely concerned with the significance of the association and recombination of ideas for purposes of discussion rather than experimentation. The more experimentally minded students of thought gradually broke away from the philosophic groups and, with some of the feelings that teenagers have for their parents, seemed to believe that anything that interested philosophers was all wrong or at least of little value. They sought brass instrument approaches, valued quantitative data, and were distrustful of philosophy.

I am trying to present some theory to explain why the modern psychologist in textbook and lecture has cautiously avoided adequate treatment of the association of ideas and their recombination in his compendium of mental processes (the new term for what philosophers had called "the faculties of the mind"). We have invented our own watertight compartments. Our elementary textbooks have separate chapters on learning, problem solving, intelligence, and often a short treatment of creative thinking. A literal-minded student might state: "At the moment I am not going to reason, use my intelligence, and do not wish to solve any problem. I plan to write a sonata and will therefore abandon myself to creative thinking."

I propose to stress the importance of the association and recombination of ideas in both noncreative and creative thinking, intelligence, problem solving, or reasoning, if you will. I will tentatively distinguish creative from noncreative activity in terms of its being original and practically or aesthetically valuable, as it may concern a new serum or song. By "original" I mean quite arbitrarily that, according to the knowledge of the world, the valued product of this activity, in part or in whole, cannot be attributed to anyone else. Very often we conclude that we have created something new until we look up the literature. The thought process is the same in creative and noncreative thinking, but the value judgment of the end product is certainly not. Again, the thought may be extremely original, yet utterly valueless, such as the ravings of a mad man. In the spirit of pragmatism, we will not include this in the category of creative thinking.

When we approach simple behavior involved in "classical conditioning" it will become increasingly difficult to observe the possibilities of discovering the

association of ideas or their recombination. In everyday or academic trial and error situations, however, extending beyond the problem box or maze, the human organism must combine remembered perceptions with perceptions of the moment or, in other words, elements that were combined with such and such, must be recombined with this and that. The operation often involves the recombination of principles and rules with data present and remembered. I am describing a kaleidoscopic operation. The larger the kaleidoscope with a greater number of pieces of glass, the greater is the number of designs that may be expected with greater complexity. The more the individual has perceived or the more the material he has collected and the longer he has spent recombining its elements, the better is the chance for a greater number and more valuable recombination of ideas. Those with better memories and a more rapid rate of association of ideas, all other things being equal, will have a better chance of success.

The *Gestalt* psychologists define "insight" as the ability to perceive the relationship between the means and the goal, which implies a recombination of ideas. I brought this fact to the attention of M. Wertheimer years ago in conversation. I also added that the same processes are involved in obtaining the wrong answer as the correct one. Hence, the *Gestalt* psychologist should differentiate between "good" and "bad" insight, or "successful" and "unsuccessful" insight. At the time he agreed with me. Perception, as it is used by this school, is broadened to include recalling what has been perceived in the past and recombining this with what might be perceived at the moment. If this is not so, then their description of learning or problem solving has no meaning.

Memory has been given more space than other mental processes, and not without good reason. The intelligence test that correlates with academic achievement must be largely a test of memory during this period of our lives when we are obliged to spend most of our time loading on board intellectual cargo.

In his discussion of "Thinking and Creativity" (elsewhere in this monograph) D. W. Taylor refers to a study showing a low correlation between the intelligence quotient and creative ability. If this conclusion is validated, I will not be surprised. We have all been shocked by finding so many high-I.Q. champions with good memories who are wretched ideational recombiners. This fact is sufficient to explain the possibility of a low correlation between good performers on the Stanford-Binet Test and creative minds who are good recombiners but not superior memorizers. A good memory, of course, does no harm. The better one's memory, the greater number of ideas he can recall, which may lead to greater discovery. As I said before, the larger the kaleidoscope with the greater number of pieces of glass, the greater number of designs may be expected with greater complexity.

When we are looking for ideas to start a literary production as students or mature adults, the association of ideas or the flow of ideas becomes of paramount importance. One Cavalier poet "bit his traunt pen" when he lacked ideas and "beat himself for spite". This might have been unnecessary had he known of tea and its capacity to prime our associational pumps. E. A. Poe, Thomas De Quincy, and others were well aware of the value of certain drugs

for this purpose. Individuals develop their own methods for promoting mental flow. Oscar Diethelm, Louis Long, and I performed an experiment at Payne Whitney Clinic, New York, N.Y., that showed that manic-depressives in an elation and medical students under the influence of dexedrine gave significantly more associations to 15 nonsense syllables than they did in the normal state.[6] This hyperassociative mental state was apparently caused by the orally ingested drug or the patient's endocrine flow. The rich repartee of the elated manic, which is absent in his normal state, is well known in psychiatry. Chapters on individual differences state that some people have a better memory than others and, likewise, that some have a richer flow of the association of ideas. Such facts are recorded even in our textbooks but, whereas countless studies have been made of memory, associative activity has been rather sadly neglected.

It is known that inhibition of the flow of association of ideas may be caused by a form of "fear conditioning" (fear of being corrected or ridiculed) and that any drug such as alcohol, which reduces inhibition although it adversely affects mentation, can be profitable. Daniel Webster never dared to make a speech without first taking a stiff drink.

The recombination of ideas naturally presupposes the ideas being available in the first place, through perceiving, recalling, association and other mental processes. Not only is time necessary but also adequate motivation, so that the activity of these mental processes is devoted to the problem with full intensity. Without the adequate ideas being present in the mind, successful recombination leading to the solution of a problem cannot be made. Even the schoolboy faced with a simple arithmetic problem is obliged to recombine what he learned yesterday with what he learned last year and two years ago, facts, principles, and formulae. His failure is usually explained in terms of his "poor reasoning ability" when very often memory failure or sufficient lack of interest to understand essential principles is the real cause. If the textbook writer knows his business, the student will have been given all of the opportunities possible to acquaint himself with the essential concepts necessary for the proper ideational recombination needed for the solution of the problem. Trial and error in terms of many ideational recombinations generally will be needed. When the boy grows much older, the recall of a greater number of both concrete and abstract ideas will be required for the solution of the mathematical, chemical, or artistic problem that no one has ever solved before. If he is successful, his work is regarded as a product of creative thinking. He has become an inventor or an artist with an original style.

Chance or a combination of accidents more often plays an important part in the process of coming upon a new idea, be it a principle, rule, or observable fact. When is an originator, an inventor, or a discoverer? In some instances this can be a very difficult question, since fortunate recombinations of ideas and lucky observations may both be involved. Sometimes originality is purely the result of, let us say, the 10,001th recombination of only past impressions, while on other occasions accident solves the problem and the discoverer observes the results. Alexander Fleming, for example, picked up a slide one morning and found that a mold had killed certain germs. In most cases of this sort, however, years or most of a lifetime have been spent by the inventor

in recombining countless ideas before he is in a position to profit by the lucky observation and to know what to do with it. Other chemists may have seen the same thing happen and thrown the cultures away as spoiled.

As experimental psychologists devising tests or constructing laboratory experiments we are likely to forget the gigantic role that time plays in the great problems of the outside world. For example, it took one celebrated pianist one quarter of his life, practicing five hours a day, to solve the extremely difficult problem of playing the way he does. J. S. Bach is supposed to have said: "Anyone can compose as well as I do if he studies as hard." Some painters have become famous because of several original elements in their techniques plus their slowly acquired skill.

Where time is essential, speed in recombining ideas is of the uttermost importance. Fisichelli and I devised an experiment to test the speed of the recombination of ideas.[2] No emphasis was placed on any aesthetic quality in our subtests because we were comparing an artist with an unselected group. Speed in ideational recombining was all that we attempted to test. In the first subtest, the subjects were required to compose as many sentences as possible from given lists of ten very familiar words. The second test required the subjects to print as many letters of the alphabet as possible with three straight lines, then one straight line and a curved line. The third test required the subjects to use in a composition as many words as possible from a list of words in the order given. The last test made use of five trials with 10 geometrically shaped blocks each, to be recombined into conventional pieces of furniture. The professional artists all made many more recombinations than any of the members of the unselected group. The quickest recombiners were cartoonists and commercial artists. Several famous portrait painters and sculptors were the slowest of the artist group. In cartooning and advertising the artist is required to recombine his ideas immediately, while the portrait painter or artist in other fields is not required to do so with the same speed or to the same extent. This is an indication of the possibility of the effect of training on recombination of ideas.

In another study using the same procedure at Hunter College, art majors were tested.[5] Their scores were not as high as the professional artists, but they were much higher than the unselected students. As we all know, only a small portion of the students who graduate from college art departments or art schools become successful artists. Why, therefore, were so many art majors quicker recombiners in this test than the unselected students? Certainly this point cannot be explained in terms of the skills acquired in art training, since no special skill was required on this test. Art majors did phenomenally better than the unselected students on the association test we gave at Payne Whitney Clinic, described earlier in this discussion.[1] Art majors gave as many responses to the 15 nonsense syllables as did the manic depressives in their elated state. There is a temptation to dismiss the subject by saying: "Oh, the art students are crazy too!" but this is nonsense. It might be more to the point to say: "Would that more art students were crazier so that more might become better artists," yet this too would be dismissing the problem with poor humor.

The only possible hypothesis I have found to explain the difference in the

performances of the art and unselected students in both tests is in terms of interests and inhibitions or lack of inhibitions found in their respective disciplines. The student, whether talented or not, who wishes to devote his life to art may be liberated in a way that others are not. The art student is encouraged from the start to strive for the new, the original product. His practice in making reasonable facsimiles of nature is but a means to the all-important end of creating in the future. Possibly such emphasis on originality breaks down inhibitions or prevents those inhibitions from being formed that are found in unselected students whose undergraduate tasks are to learn what others have created rather than attempt creation in science or the humanities. Thus the art student has the dual benefits of positive training and absence of negative or inhibiting training.

Electronic machines have been invented that can even solve problems of calculus. Certainly, most of us would have to admit that this requires reasoning ability. Just as man has been described as a "rational animal," someone might ask: "Is not such a machine similar to man in so far as it is 'rational'?" Alas, the so-called rational machine is unable to solve any problems without the fingers of a rational man trained in its operation. Of course, there would be no objection from an orthodox (John) Watsonian. If human behavior can be explained in terms of stimulus and response, so can that of a machine. The operator's fingers are the stimuli and the response, as you would expect, is a motor response. The mathematician's response is of the same type when presented with the visual stimuli representing the words and symbols constituting the representation of the problem. Obviously, Watson, although on the right track, stopped far too short when he discounted the value of the brain. By doing so he left no room for any explanation of the association or recombination of ideas. Ivan Pavlov and his disciples did; yet many of them, who were primarily interested in animal psychology, had no need to evaluate the recombination of ideas not important in a maze or problem-box situation. When, however, we come to adult problems or even the operation of a computer, the concept of ideational reorganization becomes indispensible.

I am discussing problems of higher mathematics our forefathers as "rational animals" would have respectfully and mystically explained in terms of the mental faculty of reasoning. Before becoming further involved, let us admit that the computing machine constructed to solve problems beyond the capacity of most human beings and faster and more efficiently than all of them, recombines an unconscionable number of various types of mathematical ideas, but it does so only because it has been "programmed" by man with his organizing ability. Naturally, the ideas are not contained in the machine the way they are in the human mind. The machine is filled with various signs representing these ideas, signs that we ourselves are obliged to make use of in the process of mathematical thinking. The machine has various buttons that, when pressed, bring about different processes of recombining different sets or types of mathematical ideas. The more elaborate, or shall I say the more educated, the machine, the greater the number of mathematical symbols and systems for the recombination of ideas it must possess. By the same token, the more educated the mathematician, the more symbols, rules, and formulas he will have to understand and remember. However, the mathematician is self-program-

ming. He decides on the order of his steps and then recombines, more slowly than the machine, it is true, but autonomously. He alone judges the value and implications of results.

What do I mean by "understand" in this connection? Am I slipping on a mystery word? I have not the time to go into a complete explanation, but this much can be said. A student not only remembers the symbol for two, but "understands" that it is the sort of entity that, when recombined with itself, is four, or when recombined with 100 is 102, or is one sixth of 12 when 12 undergoes the process of being divided by 6. Recombining implies division, subtraction, addition, and multiplication. This applies in any field of thought. I have seen and remembered the images of a gold watch and a snow-capped mountain. I will now subtract the gold color from the watch and add it to the form of the mountain, resulting in my thinking of a golden mountain, an object that I have never seen, a mere recombination of the quality of one past impression and the form of another. We frequently dream of persons and places we have never seen. These are the results of recombinations and so is the whole plot or scheme of most of our dreams, whatever the motivation is, Freudian or otherwise.

When we speak of a computing machine "understanding" we are asking for derision. However, is it not interesting to note that when we press the button marked "two" on the machine and press it a second time, the number "four" appears? The machine is so constructed that when the "two" button is pressed twice a process occurs that gives rise to a symbol that we understand as the symbol for four, or two times two. The machine does most of the work without caring, while we obtain all the joy from understanding the result and we alone can use the results for constructive or destructive purposes.

Before being reminded that man invented computing machines, computing machines never invented a man, I shall turn to the subject of creative thinking and its relation to machinery, where we will be plagued more than ever by ideational recombinations. However have I not already answered this question in connection with the products of computing machines? Is not the answer to a problem in calculus that no one in the world has ever solved before a product of creative thinking? No, because it is not new enough and its only real contribution was speed; it had to be programmed by man. A new composition, written in exactly the same style as Bach, is not new enough. We insist on the creator being novel before he is given much credit.

In 1949 my associates and I attempted to discover the quality of chance or accidental recombinations of musical notes and rules of harmony. Ninety-six notes written on separate slips of paper were placed in a hat. All of the notes were from a given key and there were whole, half, quarter, and eighth notes. Each note was picked out of a hat and written down. A compromise was made concerning the division of time. In one instance, the notes were divided into three-four time and in another into six-eight time, and others into different meters. One hundred bars were numbered and these numbers placed in the hat. The first 16 of these numbers drawn constituted the bars that were to be used for the musical composition. Next, basic elements of harmony were placed in the hat. The first drawn was applied to the first bar, for example, a chord on the third scale step; the second drawn was applied to the second

bar, for example, a chord on the seventh step, with additional elements being added until all of the sixteen bars were harmonized. Several three-part inventions were also constructed in this manner, making use of the rules of counterpoint.

Over 36 of such recombinations of notes and rules were made. Many were poor, but at least one half dozen were indeed pleasing compositions without any revision or editing whatsoever. One three-part invention sounded like a composition by a slightly drunk and quite modern Bach. In a lecture in 1950 to the New York Advertising Association in New York, N. Y., I played a simpler chance composition after introducing it as a composition written by Robert Schumann at the age of 17. It has a definite touch of 19th-century sentimentality, a mood that leads the listener to believe that it came straight from the heart of the composer. My audience was, of course, disillusioned when I explained that the entire composition came straight from a hat. Another composition sounded like Frederic Chopin modernized the way some of Serge Prokofieff's music might remind a listener of W. A. Mozart written in the 20th century. Still another could pass as an old Hungarian gypsy air.

A member of the Hunter College Music Department who worked with us made the remark at the end of our last session: "Don't think I'm not going to use this method of pulling notes out of a hat when I get stuck for a musical idea or motif!" Is it possible that such a mechanical method could be of any practical use to a professional composer? Sculptors often come upon an accidental design in part or in whole on a piece of stone that they carve into prominence. From time to time a valuable recombination of ideas may be observed, heard, or seen instead of resulting from associations in the brain.

One of the purposes of this experiment was to suggest a plan of how a machine could be constructed to do the work of the experimenters who drew the data out of a hat and recorded the results. The plan was simple enough, but prohibitively expensive as far as we were concerned. It consisted of a series of drums that revolved in kaleidoscopic fashion so that a metal square would accidentally pass over various microswitch buttons embedded in the base of the drum. Whenever this occurred it would set into action a short spool of magnetic tape on which was recorded notes put together by chance in a fashion we have already described. Simultaneously other drums would be activated that played a recording of the result of a chance harmonization of the same.

A recent article in *Scientific American* by Hiller[3] gives a full description of his use of the "Illiac" Digital computer at the University of Illinois, Urbana, Ill., for composing music in an alphabet code system that is later decoded by hand into conventional musical notation. Randomizations of notes and rules of harmony and counterpoint have been recombined into short melodies that he and his colleague, Leonard Isaacson, maintain are similar in style to the work of G. Palestrina. In further experiments where rules are less restricted, the results are much more *avant garde*. As Hiller describes it: "Experiments II and III thus developed the contrasts between two widely different styles, that of the 17th and that of the 20th century. One style is highly restricted and highly consonant but sounds quite simple; the other sounds dissonant, much more complex and difficult to decipher." He claims that "in the course of an hour's operation the machine produced several hundred melodies from

three to twelve notes long." In the near future he believes that the machine can be made "to produce a 42nd Mozart *Symphony*, which would prove to be a representative, but certainly almost undistinguished work."

Despite the fact that the computer is self-critical in so far as it has a "rejector" system that forces the machine to try again "until it produces a satisfactory note or until it concludes," we must wait and see if it can edit and revise after the manner of a human composer of worth. This self-criticism is only the mechanical application of critical standards programmed into the machine. Certainly the musical world will be the judge of even this critical ability, not the engineers or we psychologists.

To make a comparison between the best works of a mechanical composer and a human being is still rather in the realm of danger. I only have referred to these mechanical operations to demonstrate the important part that the recombination of ideas plays in both noncreative and creative thinking. In so many respects the best machine is far behind the capacities of an almost inferior human mind. The vast collection of impressions and their continual recombination over a lifetime is staggering in its proportion. In the ultimate analysis, however, it is just as surprising that so many billion human minds working continually produce so little. Chance affecting motivation, training, and perceptions, which in isolation should mean little or nothing, account for what we call works of genius.

The examples I have given concerning the mechanical recombination of ideas have been concerned with music, where the language or material involved deals with only eight notes in 48 keys. If we turn to art, the forms, colors, and shapes found in a realistic landscape or portrait defy such recombinations. Within recent months a few machines have been exhibited that produce an original piece of abstract or nonobjective art. Some may say that this form of painting or drawing lends itself to such devices. I shall pass on to the staggering problem of prose or rhyme. Perhaps, as it has been said, three monkeys working at typewriters could in one million years rewrite the *Bible*, but I have not the time to wait. Maybe they would improve it. This I doubt. Whether creative or noncreative operations are performed by a monkey, man or a machine, the associations and recombinations of ideas play a major role.

This conclusion, of course, raises the question, "What can be done to increase ideational associations and recombinations in thinking?" The answer is extremely difficult. Alexander Osborn in his book *Applied Imagination*[4] has a long compilation of suggestions based on reports that have been given by successful creative minds. He refers to the classic story of Jules Henrí Poincaré who hit upon the solution of an important mathematical formula while waiting for a bus. Should we stand on street corners and do likewise? Osborn tells us that "Beardsley Ruml, 'the national idea man,'" locks himself up for at least an hour a day and does nothing but muse. He describes this kind of brown study as a "state of dispersed attention," (page 160). No doubt Fyodor Dostoyevsky's passion for gambling put him into the embarrassment of debt that compelled him to write as he did in order to escape debtor's prison. Robert Burns' poetry owes much to the native wine of Scotland, whisky. What are we to conclude from these facts? Can we emulate these great people by waiting for buses, gambling, or drinking? As far as "musing" is concerned,

it stands to reason that if a creator does not take sufficient "time off" to think, to recombine his ideas, he will get nowhere. Obviously, the idiosyncrasy or accident of the genius is a part description of his situation or character, and is not a formula for someone else. There have been extraordinary ideational associators and recombiners, as well as highly motivated perceivers, remembrers, all of which go to make up that mysterious and rare complex called genius. I have also pointed out that our training should not be negative; we must not discourage or inhibit creativity in the interests of conformity.

I will begin my summation with the words of an engineer, Wolf Muller, who in a recent conversation said: "You do not get any more out of a computer than what you put into it. All it does is to recombine what you put into it." It is interesting to note that about 200 years ago David Hume said almost the same thing about the human mind. What we invest in the computer are keys representing various symbols of thought, relays, and switches and other items that make reorganization of the data possible. The mysterious human brain evolves from a one-cell organism, very slowly. At the end of nine months it is called the brain of a neonate, which is only capable of a "buzzing, blooming confusion" as William James described it. From now on over the years, this astounding living machine will accumulate what we call data by the various ways in which light and air waves affect our eyes and ears, and other stimuli affect the remaining sense organs. This is a baffling procedure indeed, but from a standpoint of efficacy, the value of this machine depends on what goes into it and how such data are recombined as it concerns creative and noncreative thinking.

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SUMMARY: OUTLOOK FOR A FUTURE PSYCHOLOGY OF THINKING

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The contributors to this monograph were selected from among those writers who had written on the psychology of thinking most recently, and the scope of the publication was determined by the aspects and problems presented by them. Naturally, it was not possible to include all whom we might have wished to include, or to present completely the present status of the psychology of thinking. Geographical location made it impossible for certain persons from abroad to participate, and personal and social factors prevented certain psychologists in the United States from accepting our invitation. In spite of these limitations, the contents of this publication demonstrate that we have been able to present what appear to be the major problems occupying present-day psychologists in the field of the psychology of the thought process. Although there may be some workers who will not find in our presentations problems with which they are most occupied, the essential problems recently dealt with in the United States are offered. It should be noted that we have consciously avoided adherence to the scope of any specific school of psychology; we have tried to widen the scope to the degree deemed necessary to gain constructive aspects for further development.

Anyone who has not limited his scientific endeavors to the advancement of some personal scientific interest or to the scheme of thought of a specific school and who is willing to give a wider horizon to his science must attempt to find a stand from which he can obtain a bird's-eye view of the basic trends and problems of the field and its tasks. One of the most valuable stands for gaining such a widened view of an aspect of science is the historical one. This is especially true with regard to our subject, the psychology of thinking, which has undergone rather unfortunate treatment by leading groups during the most recent period of developments in the field.

In turning back to the history of psychology, I fear we must cause some displeasure to the followers of J.B. Watson by going back to Descartes, whom Watson ridiculed because he misunderstood the basic Cartesian thesis. *Cogito ergo sum* was actually not "designed to save the human soul from the church fathers" (Watson, 1928), but to give modern man in his *cogito* experience (that is, in his thinking) an element of *certainty* that went far beyond any religious belief. Out of the multiplicity of human perception and experience, thinking was singled out as the surest and most objective element in human existence for bringing man to reality and truth. This, and this predominantly, makes thinking such an important factor in the human psyche, and it is the establishing of the importance of this factor that is the essence of Cartesian philosophy and psychology.

I skip the entire period of idealistic speculation and its psychology and turn to the psychologist who is acknowledged to be the founder of modern empiricism and experimentalism in psychology: Johann F. Herbart (1834), who was a psychological unitarist, believing in the totality of all psychological experience

as a unity. Separating the higher from the lower psychic capabilities (*Vermögen*), Herbart called thinking that human *Vermögen* by the aid of which experience acquires the backbone of security.

At the start of this century in the New World, no one is more representative of United States psychology of that time than John Dewey (1910). Nevertheless we find Dewey singling out thinking as the one "attitude" that appears to him chiefly physiologically associated with the human brain and proceeding mainly "in our heads." It is predominantly a special kind of thinking that Dewey singles out as "reflective thought." Thinking is again the element that provides the "evidence" of the whole of human experience; it is still the basic certainty element of our existence.

A decade later, J. B. Watson (Watson, 1928) declared that there was "no distinction between physical and mental behavior . . . that thought was a form of general bodily activity. . . and that we think with our whole body. . . ." In this postulation, thinking became removed from its previous "pedestal" position and at the same time lost its role as the factor of self-experienced certainty of the whole of experience.

The intent of this monograph would be misrepresented if it were considered as an antibehaviorist advance or an attempt to roll back the behavioristic front. The authors' statements in the introduction and the contents of the volume should make it evident that we consider the contribution of behaviorism to the development of scientific psychology of unsurpassed value. Its new, straight, and seemingly fundamentalistic approach was, when it was presented, a challenge to psychologists everywhere. It established a methodological scientific pattern that doubtless will remain as long as psychology continues to strive within the framework of scientific thinking. As in many instances in scientific history, however, the development of a specific point of view, or of a specific method, carries within itself the danger of one-sidedness, and even of misinterpretation and mistakes. Behaviorism is not free of this. If we here present criticism of behaviorism, it is only with the purpose of correcting those mistakes and giving to the important results of behaviorism their full validity and objective utilization.

Indeed, if one tests carefully the behaviorist approach, one cannot deny that it is an elementaristic one. However it proves not to be really fundamental; that is, it does not offer an autonomic certainty basis for the science of psychology, which is the final criterion for determining its validity. I (1938, 1939, 1941) am not the only one to have made intensive attempts to clarify this point. Some of the more sensitive followers of Watson have felt the lack and have elaborated on it. George Stoddard (1943), for instance, denounced Watson's "cliché" approach. However, after rejecting also Alfred Binet's old solid definition of intelligence, Stoddard came forward with his own: "Intelligence is the ability to undertake activities that are characterized by difficulty, complexity, abstractness, economy, adaptiveness to a goal, social value, the emergence of originals, and to maintain such activities under conditions that demand a concentration of energy and a resistance to emotional forces." This only places the same load on a new cart of the same make as the old, but with a coat of clouding words of fresh color. The same mistake can be found in the younger generation of behaviorists who equally felt the

lack, but still conservatively followed the path of Watson's negative attitude toward all previous attempts to find an autonomistic basis for psychology. For his part, Watson again and again threw psychology back into the arms of the natural sciences. There is no doubt that the solution of behaviorism's failures and those of psychology as such will be found by guiding it back out of the blind alley of denying to thinking its proper role as a basic psychic function and by redeveloping a sound psychology of thinking.

Let us return to J. B. Watson himself (1928), whose basic argument was that all previous psychology was subjective and introspective and its theories merely speculation. According to him, the only objective method was to observe how man behaved in a simple stimulus-response relationship. This sounds as simple as it appears sound. However, what exemplifying proofs did Watson offer?: "Every woman in this closed room will scream, stand on a chair or pull her skirts tightly around her if I turn loose ten fierce wild rats" (1928). This might have been true of Watson's class of women students or secretaries, but farm women and female naturalists, with whom I have had much association, would instead reach for a club or other appropriate weapon to exterminate the vermin. One cannot free oneself of the impression that Watson is a victim of one-sidedness and oversimplification when he attempts to reach real fundamentals, and that his theories are based even more on speculation, instead of on careful self-observation and experiment, than those he accuses of being mere speculation. We see this as we follow him, page after page, in his arguments, all of which could be challenged as half-truths and speculation. This is especially apparent when Watson widens his theoretical aspect to include predictions. For example (1928): "To make a man swell out his chest, praise and flatter him a bit." "To make a woman dress most carefully, tell her her rival is to be present at a given function." We all know that this is not, like a law of nature, true of every human specimen, but is valid only for a certain type of man or woman.

Turning to another famous behaviorist argument that uses the anthropogenetic approach (Hull, 1951), which claims to have located in the hunger cramp or hunger pain the most elementary human stimulus reaction, we find the same half-true, speculative argumentation. Any physiologist could point out that the stomach pain thus reduced to an elementary stimulus reaction could result from quite different causes: an ulcer, a chemical disturbance of the peptic juice, or a neurodermatitis of the mucous membrane of the stomach lining. We can use against behaviorism the ironic criticism that Watson himself directed against other psychologies which, he maintained, were steeped in "philosophic juice" (1928). Moreover, if we are to do more than merely describe Watson's concept, we might say that, for the sake of oversimplification and uniformity, he badly mistreated the differentiation and multitude of expression of the human nature that, fortunately, is not as simple an instrument as Watson presents it.

At one point however—the most important one in his entire concept—Watson breaks with his general uniformalism. This occurs when he deals with some factors related to the human faculty of thinking that he has tried to mastermind out of the picture. It is here that he suddenly becomes dualistic in regard to man's general experience, dividing it into the world of objects and

the world of words. Watson emphasizes that "man is the only animal which has these two" (1928). This "learned . . . world of words" is actually the only thing that remains of the general creativity of our psyche; this is thinking, which Watson describes as "inward verbalization" made to "implicit behavior." With this, Watson acknowledges that the inner world of words concerns man more than does anything else. This is a kind of shadow of the previously existing certainty experience resulting from thinking. Here also, however, is where the entire behaviorist concept lifts itself out of its angles. It is here that Watson becomes unsure regarding the method of proving his argument. Discussing loss of memory, he wrote (idem), loss of memory results "from a gradual breaking up of certain motor systems which were built during the learning process." Doubtful speculation here replaces the simple "descriptive explanation," which was postulated as the basic behavioristic principle.

Having laid himself open to such a weak point in his behaviorist concept, Watson's arguments regarding the denial of thinking, and all that concerns its role in man's experience, demand a justification. It will no longer be difficult to understand the direction that must be followed in making the corrections and additions that we believe behaviorism requires. We should like again to emphasize that we believe that, with its basic methodological formulation, behaviorism has made a permanent contribution to psychology. We know that it will not be easy for the dogmatic followers of Watson to accept any major corrections, since these must be in the direction of changing the most challenging features of behaviorism: its postulation of an objectivity—mostly experimental scientific method—superior to any application of introspection. Watson himself, however, occasionally showed that he felt his radicalism in this respect was exaggerated. He speaks occasionally of the "world of words" as "being carried along by us" and an experience in which "one's self silently speaks." Such utterances are the result of some kind of self-observation or of introspection. Indeed, Watson seems, in this area, rather unsure and unclear. One might call this the "Achilles heel" of behaviorism: this unsure attempt to make a definitively clear observation regarding the actual process of experience, including factors related to human thinking. Here we come to the point where we must widen the aspect beyond behaviorism, since we now face the field to which other schools of psychology have devoted considerable attention and achieved most important results. I refer to the school of the psychological phenomenologist, and especially Richard Hoenigswald (1925) who, a quarter of a century ago, published his 400-page volume on *Grundlagen der Denkpsychologie* (Fundamentals of the Psychology of Thinking), which is to date the most important work in this field and unfortunately is almost completely unknown in this country.

Hoenigswald made a most amazing attempt to analyze the actual function of thinking in the existence of man by a painstaking discussion of what Watson discarded with an ironic phrase: "the two-thousand-year-old philosophical-idealistic tradition of the epistemological and phenomenological meaning of all philosophical speculation." He then proceeded to a further phenomenological analysis of what psychology, since Herbart, has tried to establish about the nature of man's ability to think and the role of this faculty in relation to the other elements of our psychic life. In this, Hoenigswald had help from other

psychological schools: those of Würzburg and Leipzig, known as the "relationship," or *Ordnungs*, psychology, of which Hans Driesch (1922, 1923, 1927) was the most prominent representative.

Without using utilitaristic pressure, Hoenigswald comes to the conclusion that a real psychology cannot be a natural science but must be what it alone can be: a real autonomic psychological science that, of course, needs a special certainty principle. Rediscovered and reestablished, in a psychological-phenomenological way, this certainty principle proves to be the faculty of thinking. Hoenigswald denies that it is subjective introspection that establishes the scientific role of thinking. It is a major task of his presentation to prove that there is also an objective scientific form of psychological observation. In this, Hoenigswald is a forerunner of C. G. Jung (Harms, 1960), although he does not go as far as does Jung in asking for an actual objectivized introspective sense of observation. The main result of Hoenigswald's work is that it established thinking as the basic human psychological faculty: "The fate of psychology as a science is a proper psychology of thinking . . . thinking is the actual problem of psychology as a science." On the basis of phenomenological psychology, Hoenigswald returned thinking to the role it was given by Descartes: the actual and specific factor of certainty in human existence.

We who stand on a more or less sceptical ground regarding the role of thinking as created by Watson and the system of behaviorism believe that the results achieved by Richard Hoenigswald have somewhat the character of a distant signpost that we might use as a pointer in the right direction or as a yardstick in the attempts that, from the various levels of psychological knowledge and understanding we represent or about which we seek information and clarity, we may make towards clarifying what thinking means or ought to mean. What is offered in this volume represents the present status of such approaches.

J. P. Guilford, whose paper begins this monograph, provides the keynote in his introductory statement that "no psychological problem is more important than that of thinking." In bringing the participants to a completely new methodological platform of logic and factor analysis, Guilford paves the way toward a course of "new directions." The main theme of his paper is the need to find, for thinking and its psychology, a new, adequate basis of operation in what must be designated as actual psychological categories. It was at the start of this century that the Danish psychologist Harald Hoeffding realized that the advance of psychology to a well-founded and theoretically autonomous science would depend on breaking the Kantian philosophical categorization that is today still considered the only acceptable theoretical basis of the natural sciences. Hoeffding approached his task by writing a number of extensive studies on categories, of which the one on *Totality* (1908) gained enormous influence and was instrumental in important scientific advances, for example, biological holism. Hoeffding however never advanced to an attempt at a systematic approach to the problem of real psychological categories. This problem was clearly seen, perhaps for the first time, in the United States by Coleman Griffith (1943), but Griffith also failed to go beyond the point of indicating the need for advance along these lines. Guilford's paper represents the first attempt to present such a psychological categorical system. It gives hope

for the start of a sound epistemology of psychology, and at the same time provides a scaffold for proper systematic work on a psychology of thinking.

Jerome S. Bruner has made the important contribution of bringing together, out of the vast psychological literature, those problems—which he calls classical—that he believes are the most important to consider with a view to a more far-reaching and constructive approach to a psychology of thinking. The constructive character of Bruner's contribution consists in the attempt to fuse together the basic aspects of today's most widely accepted psychological attitudes regarding thinking with other aspects that come from dogmatic concepts of research regarding human intellectual activity. In attempting to answer the question how thinking organizes itself—using language as example—Bruner comes to his most important proposal: "A new psychology of thinking must use more powerful descriptive tools deriving from logic and linguistics with the aim of formulating a prescriptive concept of thought reproduction." In this connection Bruner demands a new approach to the problem of memory and reproductivity in the direction of "leveled own organization and a structural understanding of the contents of the thought process." Bruner's third major proposal is to ask for a breaking down of the meaningless border between the established patterns of language and myth, which apparently have a collective character, and the process of individual thinking.

In an incidental ironic remark, Bruner indicates that he had sometimes to apply introspective observation; this admission points up one of our major problems, namely, whether and how to break through the solid wall of unsound, so-called experimentalistic objectivism and how to develop reasonable and sound "objective" methods, including, to the necessary degree, what we today may still want to call introspection but which a later generation may designate as a sound, objective, and scientific approach to psychology.

Kurt Goldstein's name has for decades been connected with work on brain localization, neuropsychiatric borderline problems, and the wider aspects of psychophysio relationships. Speech and thinking defects have been a major field of his applications. Goldstein's synthetic way of thinking, as expressed in his writings, make him a unique contributor in offering basic aspects for bringing the problems of speech and thinking into new lanes of advance and development and out of the grip of dogmatic theorization. Although one might want to view the relationship of language and thinking on the basis of scientific background as it has been developed by Fritz Mauthner, Benno Erdmann, Otto Jespersen, and Ferdinand Saussure, the medicopathological approach applied by Goldstein appears especially helpful in dealing with discussions and contradictions based not upon psychological or cultural concepts, but on natural science concepts.

Tracy S. Kendler's paper is interesting for the scientific problematic expressed and for the specific and no less interesting presentation of aspects of development of juvenile thinking. The scientific problematic lies in the difficulties facing the behavioristic theoretical aspects when there is an attempt to step out of a routine pattern into a sincere attempt to activate serious new results. The difficulties Kendler encountered and her emphasis on the lack of "an adequate measuring instrument" indicate the direction in which the search

for a "new psychology of thinking" must endeavor to advance. The discussion at the end of Kendler's paper should be taken most seriously since it lays open the most important aspects and questions in this area.

Donald M. Johnson's contribution is an attempt to get at fundamentals of our problem by studying thinking in what appears to him its purest appearance: "episodes of concentrated intellectual activity." The presentation is a keen attempt to produce experimentally, before our observing eyes, entire problem-solving episodes in terms of sequential thought processes. Johnson reports in detail the methods applied and the results obtained, as well as the problematics encountered in such novel experiments.

W. Edgar Vinacke has undertaken the difficult task of surveying and sifting the considerable literature on motivation conditioning in so far as it relates to thinking.

Motivation conditioning is probably the most important background aspect of any study of thinking, and Vinacke has attempted to envision its complexity. Against the one-sidedness and dogmatism frequently found in the literature, he has posed what one might call a pluralistic point of view. The sensitivity with which he has outlined the various aspects makes his paper a unique basis for general study of motivation as well as for our special problems.

Charles N. Cofer has taken the persisting method of experimentation along the behavioristic line and applied it to verbal processes in relation to concept formation and problem solving. He represents, therefore, the standpoint diametrically opposed to that of Goldstein. The interesting and important fact is that in both papers the problematic aspect coincides. Cofer states: "Problem solving can be influenced by associative patterns among words pertinent to the problem and by vocalization of the steps taken to work on the problem, and solution of verbal problems has been shown to be a function of general linguistic properties like word frequency and letter sequence." That research efforts undertaken from different points of view should end up with the same results appears to me one of the most important indications of advance in the confused and even deflated field of the psychology of thinking.

Donald W. Taylor's paper impressively brings out the lack of even the most basic aspects regarding creativity of thinking in the scope of present-day psychology. There is an almost endless amount of material on creativity of thinking processes in the philosophical and epistemological literature. Strange as it may appear, psychologists find these materials inadequate even as springboards to the psychological approach. It is hoped that at some time the results of the phenomenological and existentialist school—especially as represented by Heidegger and Leon Brunschvicg—will at least be examined to determine whether any tangent of them may have bearing on basic psychological issues.

Taylor has made it his task to stake out, psychologically, the aspects in the study of creativity that relate to thinking. He has designated the aspects as those of product, process, task, personal characteristics, and environment. He has also made a beginning in demonstrating how interrelationship and coordination are forces developing creativity. One might call Taylor's contribution a layout study based on the conceptional point of view he represents.

Abraham S. Luchins has presented another aspect of the creativity of think-

ing, one that might be called the problematic one. Luchins points to the fact that most creativity studies have centered on the creator, that is, the creative individual. He calls for what he designates the phenomenon-centered point of view, that is, the study of the creative act, and his paper reports on various advances made toward this approach.

Livingston Welch's humorous contribution undertakes to refocus the view of human creativity outside the range of the computing machine. His basic approach is again to bring the study of intellectual creativity nearer to the actual previous philosophical approach, as something "original" in the human thought process. His presentation, the purpose of which is to revitalize and find new directions for work in the field of the psychology of thinking, is an eminently suitable ending for this monograph.

The materials brought together in this monograph of individual presentations center around four topics: theoretical aspects, developmental problems, measurement, and creativity of a psychology of thinking. These topics were selected because they appeared as most essential ones, if we want to acquire, fundamentalistically, a solid ground in our particular scientific field.

The closing words of this summary ought to be devoted, to make them longest remembered, to an expression of thanks to Margaret E. Tresselt, who did more than anyone else toward achieving the publication of this monograph.

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